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THE MAKING OF A PHYSICIST

THE physicist, like the poet, is born and not made. Neither can be produced, on demand, by any system of training. In each category we have to make do with what Nature, in her wisdom, supplies, and the number in each category is extremely small. As regards the physicist, statistics have recently been given by Sir Lawrence Bragg in a discourse on "Physicists after the War", delivered at the Royal Institution (NATURE of July 18, p. 75). If, following Sir Lawrence, we define a good physicist as "a man capable of independent thought, with a flair for his subject", then in Great Britain "about one good physicist is bred per year per million inhabitants". Assuming that each has a working life of thirty years, this gives us a total of some fifteen hundred professional physicists, an estimate which agrees fairly closely with the records of the Central Register. It is interesting to note that a preliminary survey made some months ago in the United States in preparation for its war effort yields substantially the same proportion. It is possible that a few potential physicists escape the widely flung net of our scholarship system and remain "mute, inglorious Miltons", and it is certain that a small number, owing to accidental circumstances, find themselves engaged as chemists or engineers, but it is unlikely that the grand total of available physicists falling within the scope of Sir Lawrence's definition can be much increased. The total falls much below our present war-time requirements, and well below the requirements of a post-war society in which science will play its proper part.

Fortunately, the situation is not quite so desperate as it seems. The standard set by Sir Lawrence's definition is a high one—there can be few, if any, other professions which make such rigorous demands of their membership—and many of the tasks for which physicists are required, both in the present emergency and in the normal routine of industry, can be very adequately performed by men and women less well gifted. In an attempt to increase the number of young physicists for war purposes, the Board of Education last summer made the experiment of offering State bursaries on a generous scale to all boys and girls reaching a definite standard in physics and cognate subjects in the Higher School Certificate examinations. This has resulted in a very substantial increase in the size of the physics classes in the universities and colleges. The experiment is too recent to allow of a final pronouncement ; a preliminary survey makes it doubtful whether it will add materially to the limited number of "good physicists". There is, however, no doubt whatever that it is adding substantially to the numbers available for the more routine but no less important posts for which originality of a high order is not a *sine qua non*. It may be safely predicted that there will be many such posts in post-war industry ; and these new recruits form a valuable reinforcement to our scientific potential. Their existence will enable us to assign the always strictly limited number of men with a real flair for research to work which they alone can do.

Even with this addition, our assets, in scientific man-power, are small. It is worth while, even in war-time, to spend some sustained thought on the best methods of training and utilization. For the physicist, indeed, the problem is one of particular urgency. To meet the immediate demands for young physicists in the war effort, university courses in physics are being not only curtailed but also rather heavily biased towards specific parts of the subject of immediate application. Thanks to the wisdom and foresight with which the "Hankey training scheme", as it is generally called, has been planned and administered, the courses remain recognizably courses in physics. Broad foundations are being laid, upon which further training can be based when the present emergency is over. It must be recognized, however, that most of the young physicists now hurrying from the universities to take their place in the national effort are far from being fully equipped for the scientific work which they will be called upon to carry out, we may hope, in the post-war reconstruction. If the irreplaceable asset represented by their skill and intelligence is to be used to the full their education must be completed, and their training for new tasks undertaken. There is reason for believing that this necessity has not escaped notice in official circles. Since arrangements, to be effective, must precede the demobilization of the men and women concerned, discussion of the best methods for the education and training of physicists is one of immediate urgency, and the Institute of Physics has been well advised in placing the subject well to the fore on the agenda of its Planning Committee. A memorandum on the subject, prepared by Dr. H. Lowery, has already been circulated to members of the Institute and others interested in the subject. This, together, no doubt, with the very interesting and novel views expressed by Sir Lawrence Bragg in the lecture to which reference has already been made, will serve as the basis for a discussion which the Institute is hoping to hold in the early autumn.

Without venturing to anticipate the results of such discussion, it may be useful to direct attention to a few general principles, since general principles have an uncanny habit of getting mislaid when attention begins to focus itself on details. In particular, the fundamental distinction between education and training is one which is very apt to become blurred. (There is evidence of this in reports of recent, otherwise inspiring, speeches of the President of the Board of Education.) The distinction is of particular importance when dealing with the making of a research physicist. Though his discoveries sometimes lead to considerable advances in engineering, the research physicist is not a kind of super-engineer; in fact, industrialists who try to employ him on engineering work usually find him distinctly disappointing. His real affinity is with the creative artist, and his value to the community lies in the spark of originality which he brings with him into the world. To fan this spark into a flame, to feed it with appropriate fuel, or, to drop the metaphor, to assist the student to develop his full stature as a physicist is our first and most urgent task. Training for a more specific task should follow, not accompany, this process of education. Since the distinction between the "good physicist" and what we may call the routine physicist is one of degree rather than kind, it would appear that the same process should also be applied to the latter, up to the limits of his capacity.

Education, at its best, is a very personal matter, not lightly to be intruded upon by outsiders. The student gains more from contact with the mind and personality of his professors than from their formal instruction. That is one reason why the university department with its happy blend of teaching and research embodied in the same individual affords, on the whole, such an excellent cultural medium for the embryo physicist. There is, of course, no room for complacency. Lacking the centuries of experience which have gone to the perfecting of, say, the teaching of the classics, and faced with the difficulty of teaching a subject which is expanding at an enormous rate, it is not to be expected that physicists should have yet discovered the best ways of presenting their subject. In one particular a grave error has undoubtedly developed. In the attempt to keep pace with the rapid growth of the subject, far too heavy a load has been placed upon the shoulders of the 'honours' physics student. From the moment when he is identified as a possible scholarship candidate to the moment when he emerges dazed but triumphant as an honours graduate, his time has been completely absorbed in his subject. He has not had leisure in which to grow up. "He has not", to quote Sir Lawrence Bragg again, "learnt to mix with other people, and form one of a team, to take responsibility, to trust his own judgment, and have initiative. . . . The industrialist compares him unfavourably with boys who have worked their way up, and become men of the world."

The picture is a little overcharged, and it would be possible to quote many exceptions. That there is much substance in it cannot be doubted. Considerable doubt may be felt as to the efficacy of Sir Lawrence's suggestion that the problem could be solved by interpolating a six months apprenticeship to industry for all honours students between their leaving school and entering the university. Apart from the fact that many educationists would regard a definite break in the student's career at this stage as being undesirable in itself, there is the risk that this preliminary apprenticeship might lead the student to regard his subsequent studies as a training for industrial service, rather than as an education in physics; and to be a little impatient (anyone who has had to teach physics to medical or engineering students will know the symptoms) of matters of no obvious practical or industrial importance. But it is precisely because the physicist, for the sheer love of the subject, has studied many matters of no obvious practical importance that he is able to make his unique contribution to industry. The fact that the charge of immaturity is not, as a rule, levelled at the statesman, barrister, physician or economist who graduates through the universities of Great Britain shows that there is little wrong with the environment itself. The trouble is that, owing to the pressure of work, the

physics student has been largely isolated from this environment. The remedy is to lighten the load, and so give him the opportunity to take an adequate part in the non-academic but formative activities which are such a valuable part of university life.

This necessary reform cannot be carried out in isolation—no university will willingly admit that its honours standard is less exacting than that of its neighbours—but it should not be beyond the powers of the universities as a whole to work out an agreed solution. As regards the schools, much might be achieved by the elimination of the present competitive scholarship examinations and the substitution of a system on the lines of the new State bursaries. If every pupil who reached a certain well-defined standard was assured of a university education if he desired it, much of the stress which hampers the real education of the present scholarship candidate would disappear. Any extra expense in which the State might be involved would yield a rich return.

The training of the young graduate for research is generally provided for by allowing him to spend a year or two in research in the university. The system has grown up quite fortuitously, but its value has not been seriously challenged by the industrialist. It would probably be difficult to devise anything better. One proviso, however, must be made. It should be clearly kept in mind by the student, and particularly by the director of research, that these years are years of training. Their object is to produce a research physicist and not a series of "communications". The neophyte should be given a problem which will test his powers, and considerable latitude in handling it. The freedom to make one's own mistakes, though not formally embodied in any charter, plays a valuable part in the process of training. The temptation to use the trainee as an observer in other people's researches should be firmly resisted, and the department should be satisfied to be judged primarily by the quality of the men it produces, rather than by the quantity of original papers it turns out. In this connexion the methods of Sir J. J. Thomson, the greatest trainer of research physicists the world has yet seen, are worth close study.

On the psychological or personal side, it must be confessed, the position is not quite so happy. While the routine physicist, as a rule, settles down quite readily in industry, the research physicist only too often both experiences and creates a certain irritation. The industrialist complains that his new recruit has far too good an opinion of himself. The young physicist, eager to prove his worth and to give of his best, finds himself enmeshed in a system of regulations and restrictions which appear to him to have been specially designed to prevent him from getting on with his work, and very often called upon to do things which seem to him of negligible importance. There is need for adjustments on both sides, if physics is to make its maximum contribution to the welfare of society. Not only must the physicist learn to fit himself for industrial conditions—industry must also learn to adjust its conditions to the physicist. Goodwill is not lacking on either side. The physicist in industry is as keen upon his work, and as rightly convinced of its value, as his university colleague; while our best industrial concerns are genuinely proud of their research departments and anxious for their welfare and efficiency. What is needed is a better understanding of the conditions which are essential to the creative artist (let us strike this note again, for it is the keynote in the situation) if he is to give the best of which he is capable. It may be admitted that the problem of making the necessary adjustments in organization is not an easy one for the industrialist; it requires courage and vision, qualities in which the great industrialist should not be lacking. Progress has been made, particularly in the United States; and some of the larger industries in Great Britain are at least moving in the right direction, with Government research, on the whole, lagging sadly in the rear.

The problem of the best utilization of our limited scientific capital involves more than the provision of the best methods of educating and training the scientific worker himself; the conditions under which his special gifts can find their amplest outlet must simultaneously come under review. It is only as the two aspects of the problem are studied in conjunction that the right solution will be attained.

THE HIPPOCRATIC MYTH

Disease and the Social System

By Arthur Guirdham. Pp. xvi+240. (London: George Allen and Unwin, Ltd., 1942.) 10*s.* 6*d.* net.

IT is a pity that Dr. Guirdham does not value his ideas more highly, since many of them are exceedingly good. To recapture the primordial Hippocratic vision, in which specific disease is seen in relation to the total personality, is an idea worthy of serious handling. But the author's staccato, rather malicious, style fails to clothe his ideas with persuasive interest. The satirical mood is convincing only when the author is truly gripped by his vision. But for satire to be effective the cheap sneer must at all costs be excluded. In this work even the chapter headings are not free from this curious vulgarity as, for example, the caption "Lead Kindly Light", above a chapter in which current medical teaching is held up to ridicule.

In regard to his main theme Dr. Guirdham is clearly right when he tells us that we have lost the true perspective, that we no longer see the total human being in our teaching of medicine, but have apportioned man into arbitrary fields for specialists. It must be admitted, however, that the idea was presented better by Plato and with greater economy of argument. In the Dialogues, for example, we find: "But Zamolxis . . . says further that as you ought not to attempt to cure the eyes without the head, or the head without the eyes, so neither ought you to attempt to cure the body without the soul: and this, he said, is the reason why the cure of many diseases is unknown to the physicians of Hellas, because they are ignorant of the whole, which ought to be studied also; for the part can never be well unless the whole is well." Plato then proceeds to affirm the importance of

psychotherapy as the fundamental medical approach. "For all good and evil," he asserts, "whether in the body or in human nature, originates . . . in the soul, and overflows from thence, as from the head into the eyes. And, therefore, if the head and the body are to be well, you must begin by curing the soul; that is the first thing." In this, the very foundation of the true medical point of view, Dr. Guirdham's illumination has not the clear integrity of the Greek vision whence his inspiration is admittedly drawn. Indeed, he reserves his worst venom for modern psychotherapeutic methods, without himself providing any better means for grappling with the problems of the soul than that of masterful hortation.

If Dr. Guirdham's main contention can be substantiated, namely, that the incidence and character of disease have shifted in the direction of the autonomic, endocrine and psychical fields, then the problem of disease in the future will surely demand the fullest possible research into the fundamental psychological determinants of human nature. The historical patterns which determine the character of our social system are part of our psychological inheritance and, since the individual is the unit of society, it must be in the realm of the individual psyche where the tyranny of the pattern can be fought. Logically, therefore, if Dr. Guirdham were to take his ideas seriously, he would be advocating far-reaching research into the nature of the unconscious. He would also insist upon the teaching and practice of psychotherapy as an integral function of the health centres which, in the near future, will determine the character of medical practice. It is just because he breaks his lance against the social windmill that the author fails to convince either himself or his reader that he has any effective remedy for the ills of mankind. His snap judgments and ineffectual satire might even be regarded as the product of Quixotic frustration. He condemns modern man for chasing vague and nebulous goals, but is not the social system, which he himself attacks, the most nebulous goal of all?

The book abounds with dashing generalizations of which the following is a fair specimen: "Psychiatrists should also be historians. The German race for two thousand years have maintained a carefully conceived national policy of brutality, fraud, opportunism, cunning and terrorism. They have lived in the continent which above all others has been throughout the centuries a pioneer in civilization." For how long has the German race been a nation with a "carefully conceived national policy"? What student of history would put his name to such an assertion? Moreover, one of the main reasons for our inability to prepare against the German cataclysm was because we could not believe that Germany could ever be guilty of destroying the very civilization of which she herself had always, and rightly, claimed to be a pioneer. Our present confusion of mind in regard to post-war problems is not unrelated to this suicidal split in German psychology. It is difficult to accept that the upper half of a nation can be civilized while the lower half remains sunk in barbarism. But are we also completely civilized from top to toe? Without the historico-psychological point of view, what hope is there for a fair appraisal of the problems of our time?

Yet, in spite of the author's exasperating recklessness of statement, his book is also rich in good ideas, and if mental stimulation were all one required it would serve remarkably well. H. G. BAYNES.

TEXT-BOOKS OF ACOUSTICS

Acoustics

By Dr. Alexander Wood. (The Student's Physics.) Pp. xvi+588. (London, Glasgow and Bombay: Blackie and Son, Ltd., 1940.) 27*s*. 6*d*. net.

A Text-book of Sound

Being an Account of the Physics of Vibrations with Special Reference to Recent Theoretical and Technical Developments. By Dr. A. B. Wood. Second, revised and enlarged, edition. Pp. xvi+578. (London: G. Bell and Sons, Ltd., 1941.) 30*s*. net.

THERE can be no doubt that British text-books of acoustics are the finest published in any language. Most of the best advanced text-books in physics now come from the United States. General text-books in acoustics are still the exception, and the British product is best. For this reason, Dr. Alexander Wood, in writing the latest text-book, set himself a difficult task. The result is completely successful. Modestly stating that the book "makes no claim to originality", he gives the clue to its excellent qualities by explaining that he wrote "primarily for the interest of writing".

The treatment throughout is comprehensive. Theory, experiments and applications are given with references to original sources. Many excellent diagrams, graphs and photographs are included. As the history of each branch is given more fully than in any other British text-book, the student is helped to realize that in science the branches of a subject are developed gradually, that discoveries in another field may suddenly provide just what was wanted to help in the subject of his own special interest. Because of spasmodic developments, the historical presentation of a subject may be too slow for the 'go-getting', examination-pursuing student. The method presents, however, the facts of how a subject develops and of how his own researches will go if he follows any but the path of safety-first.

Much of the beauty of physics lies in the linking together of such diverse phenomena, affecting different sense organs, by a few simple general principles. Vibrations and wave motion unite sound with mechanical, optical and electrical phenomena. The author neglects no opportunity of developing the relationships. For example, the chapter on diffraction includes an experiment in acoustics analogous to crystal 'reflexion' of X-rays. Two minor suggestions may be made. Students would be helped by a collection of problems (a few examples are given in the text) and by an annotated bibliography of the standard reference works in acoustics, including the journals specially devoted to the subject.

Dr. A. B. Wood's "Text-book of Sound", first published in 1930, has since been twice reprinted and now appears in a revised and enlarged second edition. The new edition preserves all the excellences of the old, and there is newer material in such branches as supersonics, hearing, noise and various technical applications. Where the additions do not justify resetting the text, new references to original sources are given.

If comparisons are made, the order of increasing fullness of treatment is Richardson's "Text-Book of Sound", followed by A. B. Wood's, with Alexander Wood's outstandingly mature work giving the fullest treatment of the subject available in English.

W. H. GEORGE.

HALLEY'S COMET AND ITS IMPORTANCE*

By PROF. H. C. PLUMMER, F.R.S.

THIS lecture coincides with the bicentenary of Halley's death, for he died, the second Astronomer Royal, on January 14, 1742, at the age of eighty-five. To this office he succeeded Flamsteed, after holding the position of Savilian professor of geometry from 1703 until 1720.

The quality for which Edmond Halley, Hon. D.C.L. of the Queen's College in the University of Oxford, was most remarkable was beyond any doubt his versatility, which ranged far beyond the subjects which are prescribed for this lecture. Thus in the Breslau Tables he made the first contribution of fundamental importance to the science of vital statistics. He was a post captain in the Royal Navy. He gave effective advice to the Emperor on the fortification of Trieste. Here it may be interesting to notice that in this task he was following a tradition which can certainly be traced back to Galileo who, as professor of mathematics at Padua, lectured on the subject of fortification and published a text-book on it.

But here I shall turn to what is probably the most important side of Halley's astronomical work, his discussion of the orbits of comets. This subject has not been dealt with at any length in these lectures, and it is related to a matter which might be regarded as Halley's greatest achievement, and perhaps the most difficult: the tactful handling of Newton and the production from him of his supreme effort.

The appearance of a great comet is an infrequent event, occurring at very irregular intervals. Year by year several comets, say from five to ten, are seen and observed. Some of these even become visible to the naked eye, but they are rarely conspicuous objects. The last comet which earned the title of 'great' was seen in the year 1882. It was notable as the first comet to have its portrait taken. The phrase is appropriate because the portrait was taken by an ordinary professional photographer with an ordinary portrait lens in an ordinary camera strapped to an equatorial at the Cape Observatory. The event signalized the advent of the dry plate, which was destined to re-arm the astronomer with a new weapon. Not that this was the first use of photography in astronomy—it had been employed for several years in the new Oxford Observatory—but it marked the introduction of a far simpler process by a striking example.

In the sixty years which have elapsed since that date, only two comets have excited popular attention in the northern hemisphere, though several have provided notable objects for photographic study. Both of these comets were seen in the year 1910. The first came unheralded, and many will remember its beautiful and impressive appearance in the February of that year. The approach of the other had been followed since the preceding autumn, when it was first detected well within 10′ of its predicted place. For this was Halley's celebrated comet returning from its long journey after seventy-five years absence. Because the nearest approach to the sun took place in April, the appearance of the comet was disappointing in this latitude. But in tropical countries the conditions were more favourable and the spectacle was impressive enough. The February comet will need no further remark, but that it illustrates one point. In order to establish the identity of a comet, much more is necessary than its appearance near an expected date. For the two brightest comets in the last sixty years were separated by no more than three months in the time of perihelion passage.

The history of Halley's Comet has been traced back over more than two thousand years. Of this long period barely the last third is covered by useful European observations. Happily in this instance the balance has been redressed by recourse to the East, for Chinese records have proved invaluable. This is true even in the middle of the period, when Arabian astronomy was in the ascendant. The Arabs, like Ptolemy, do not seem to have paid much attention to comets; any observations made by them seem to have escaped notice, if indeed they were made at all.

Thus the story of a single comet represents a section of astronomical history. In the ancient world perhaps Seneca came nearest to a sensible view of comets, but nothing of real scientific value was possible before the fifteenth century, when the Viennese school became active in astronomy. Its most distinguished product, Regiomontanus, observed the comet of 1472 and wrote on the subject. But his work on comets was not published during his short life-time. To him is doubtfully[1] assigned a limiting parallax which places comets beyond any reasonable boundary of the atmosphere. This deserves notice as an early result of observation. The next step was made by Tycho Brahe, who from observations of the comet of 1577 narrowed the parallax to 1°, and so placed comets beyond the moon. It remained for Kepler, observing the comet of 1607 (Halley) to go further and assign the orbit to planetary space. Thus the seventeenth century started with a fair knowledge of the region in which comets are located. This does not mean that so much was universally accepted: Galileo, for example, entertained ideas which were quite retrograde, even after 1618, when three bright comets appeared.

If sound views on distance were slow in forming, naturally ideas on the nature of cometary motions were even more backward. Tycho supposed the motion circular, and Kepler, confronted by a problem apparently quite unlike that presented by the planet Mars, conjectured motion in a straight line. We shall notice later on that a much sounder view was taken far nearer home. Hevelius in his "Cometographia" (1668) suggested a parabolic orbit, but this was a vague guess which had been anticipated by Borelli (alias Mutoli) even earlier (1664). Dörfel, who had been a pupil of Hevelius, arrived by a more legitimate path at the same conclusion as his master, by applying a geometrical discussion to the observed places of the great comet of 1680. But this work, whatever its merit, came too late to have any real importance. Astronomy was already on the brink of its most momentous epoch, and the true solution of the problem was now so near at hand, and as we now know so inevitable, that more or less fumbling guesses lose their interest. The law of cometary motion was never discovered in the sense that Kepler discovered the laws of planetary motion. The knowledge came simply as a by-product of Newton's work on gravitation.

Towards the end of the year 1680 the comet already mentioned was discovered by Gottfried Kirch at Coburg in Saxony. Few observed it before it

* Halley Lecture delivered at the University of Oxford on May 25.

reached perihelion; afterwards there were as many observers as astronomers living[2]. This was the comet observed and discussed by Dörfel, a pastor at Plauen in Saxony, in a work which has become exceedingly rare and inaccessible. It was also observed by Newton, and thus helped to revive in him an interest in astronomy which had been dormant for many years. But Newton's experience was not altogether happy, as the following extract from a letter indicates.

Newton for Flamsteed (through Mr. Crompton).
"Cambridge, Feb. 28, 1680.

"I am further suspicious that the Comets of November and December which Mr. Flamsteed accounts one and the same comet were two different ones, and I find Cassini in a copy of a letter of his which Mr. Ellis shewed me is of my mind. . . ."

Of course, Flamsteed was right and Newton was wrong, though he erred in respectable company. But the sequel is important enough to warrant this quotation.

Newton to Flamsteed.
"Trinity College, Cambridge.
"Sept. 19, 1685.

"Sir,

". . . I have not yet computed the orbit of a comet, but am going about it, and taking that of 1680 into consideration, it seems very probable that those of November and December were the same comet . . ."

It was convenient to forget the letter of five years before, but fortunately we are not concerned with that aspect of the case. By this time the acid stimulus of Hooke and the urgency of Halley have done their work, and Newton had already made substantial progress in the preparation of the "Principia". But though the principles were established and the complete design no doubt formulated, the necessary details for calculating the orbit of a comet were clearly not worked out before the end of 1685.

The outcome of these hectic months in Newton's life is well known. In the "History of the Royal Society of London"[3] it appears thus:

"1687. April 6, Dr. Gale, vice-president, in the chair.

"After the minutes of the last meeting were read, the third book of Mr. Newton's treatise *De Systemate Mundi* was produced and presented to the Society. It contained the whole system of celestial motions, as well of the secondary as primary planets, with the theory of comets; which he illustrates by the example of the great comet of 1680–1, proving that, which appeared in the morning in the month of Nov. preceding, to have been the same comet, that was observed in Dec. and Jan. in the evening.

"Dr. Gale reported that he had lately received a letter of thanks from the authors of the *Acta Eruditorum Lipsiensia* for the *History of Fishes* sent them as a present from the Society."

In view of the importance of the occasion, the stress laid on a detail in cometary history may seem exaggerated, but is nevertheless significant. Similarly, there is a reason for repeating another single item from the minutes of the same meeting. The publication of the "History of Fishes" had gone far to deplete the funds of the Society and made it difficult to carry out another costly undertaking. How Halley stepped into the breach and took on himself the liability for the production of a work which owes so much to his encouragement for its existence is a familiar story. It is impossible to conjecture what the world would have lost if Halley had not lived. He had done more than anyone else to inspire the "Principia", he had kept an over-sensitive author to his task, and now he undertook its publication. The nature of a fourth service is still to be seen.

Several years passed, and Newton was within a year of leaving Cambridge, when more is heard on the subject of comets. The latest development can be given in Newton's words:

Newton to Flamsteed.
"Cambridge, Sept. 14, 1695.

"Sir,

"When I received your last, Mr. Halley was with me about a design for determining the orbs of some comets for me. He has since determined the orb of the comet of 1683 by my theory; and finds, by an exact calculus, that it answers all your observations and his own to a minute. . . ."

The reference is clearly to the comet of 1682, now known as Halley's, but the great man was ever a little careless about dates. This work on which Halley was already engaged only saw the light ten years later. His time was fully occupied. Between 1696 and 1698 he was deputy controller of the mint at Chester. In the four years after that he was almost continuously at sea in command of H.M. Pink, the *Paramour*, first in making the magnetic survey and then in charting the English Channel. Next year (1702) he made two voyages to the Adriatic, to advise a foreign Government on the fortification of Trieste. At last, in 1703, he became Savilian professor of geometry, in succession to Wallis, and could look forward to a time of comparative leisure. What he did was to finish the work on comets, and when that was published he produced in the following year the edition of Apollonius, for the purpose of which he had learnt Arabic. His activity at this period of his life was certainly amazing.

The paper[4], "Synopsis Astronomiæ Cometicæ", in which Halley communicated the results of his cometary researches to the Royal Society, is a model which can be commended to the attention of those authors who find that scientific societies are unwilling to grant all the space which they think is due to them. For this memoir, the importance of which has been universally recognized by laudatory epithets*, fills less than eighteen small quarto pages of fairly large type. There is a historical introduction (3 pp.). A single page contains the kernel of the whole, a table giving the parabolic elements of twenty-four comets; as Halley says, "Immensi pene Calculi fructum, exiguum quidem . . . munus." Then follows a kind of Barker's Table† (4 pp.) with a description and example of its use occupying about the same space. It can be inferred how much room was left for a discussion of the results.

In this paper purely parabolic motion is assumed throughout. The orbits are determined by Newton's methods. Whatever was not to be found or not understood in the "Principia" would naturally be supplied by Newton, for the two men were in close contact in this work, as the letter of 1695 shows.

* For example, "epochemachend" (Mädler); "ces travaux si brillants et si neufs" (Delambre).

† The original Barker's Table was first published in 1757.

There has never been any dispute about their shares in the result, not need there be. Newton provided the methods, which from a practical point of view left ample room for improvement*, and Halley did the work of calculation.

PARABOLIC ELEMENTS OF 24 COMETS. HALLEY

Date of Perihelion Passage	Ascending Node	Inclination	Motion	Perihelion Longitude	Perihelion Distance
1337 June 2 6 25	84° 21′ 0″	32° 11′ 0″	R	37° 59′ 0″	0·40663
1472 Feb. 28 22 23	281 46 20	5 20 0	R	45 33 30	0·54273
1531 Aug. 24 21 18	**49 25 0**	**17 56 0**	**R**	**301 39 0**	**0·58700**
1532 Oct. 19 22 12	80 27 0	32 36 0	D	111 7 0	0·50910
1556 April 21 20 3	175 42 0	32 6 30	D	278 50 0	0·46390
1577 Oct. 26 18 45	25 52 0	74 32 45	R	129 22 0	0·18342
1580 Nov. 28 15 0	18 57 20	64 40 0	D	109 5 50	0·59628
1585 Sept. 27 19 20	37 42 30	6 4 0	D	8 51 0	1·09358
1590 Jan. 29 3 45	165 30 40	29 40 40	R	216 54 30	0·57661
1596 July 31 19 55	312 12 30	55 12 0	R	228 16 0	0·51293
1607 Oct. 16 3 50	**50 21 0**	**17 2 0**	**R**	**302 16 0**	**0·58680**
1618 Oct. 29 12 23	76 1 0	37 34 0	D	2 14 0	0·37975
1652 Nov. 2 15 40	88 10 0	79 28 0	D	28 18 40	0·84750
1661 Jan. 16 23 41	82 30 30	32 35 50	D	115 58 40	0·44851
1664 Nov. 24 11 52	81 14 0	21 18 30	R	130 41 25	1·02575,
1665 April 14 5 15	228 2 0	76 5 0	R	71 54 30	0·10649
1672 Feb. 20 8 37	297 30 30	83 22 10	D	46 59 30	0·69739
1677 April 26 0 37	236 49 10	79 3 15	R	137 37 5	0·28059
1680 Dec. 8 0 6	272 2 0	60 56 0	D	262 39 30	0·00612,
1682 Sept. 4 7 39	**51 16 30**	**17 58 0**	**R**	**302 52 45**	**0·58328**
1683 July 3 2 50	173 23 0	83 11 0	R	85 29 30	0·56020
1684 May 29 10 16	268 15 0	65 48 40	D	238 52 0	0·96015
1686 Sept. 6 14 33	350 34 40	31 21 40	D	77 0 30	0·32500
1698 Oct. 8 16 57	267 44 15	11 46 0	R	270 51 15	0·69129

A glance at the table of parabolic elements, which is reproduced here in a slightly modified form (in the original the longitudes are naturally expressed in the conventional Zodiacal signs), is enough to show the main result. The orbits found for the comets in the years 1531, 1607 and 1682 are strikingly similar. For this fact there are two possible explanations. Either three different comets follow the same path at nearly equal intervals of time (seventy-six or seventy-five years), or else it is one and the same comet describing an elongated elliptic orbit which would differ little from a parabola in the neighbourhood of the sun. Between these alternatives the question must have been one of probability. More recently, groups of comets with similar orbits have been found. But when the whole number of known orbits is no more than a couple of dozen and the difference in period is only a year, the choice looks simple. Still there is that difference, and therefore there is an element of risk. The reason for it Halley quite rightly attributed to the disturbing influence of the planets.

When the paper was reproduced with additions ten years after the author's death in 1742, in "Dr. Halley's Astronomical Tables", more confidence is shown in the identity of the three comets. Motion in a highly eccentric ellipse has been investigated. The mean distance assigned is $a=17{\cdot}8635$, the eccentricity (in Halley's sense) is $ae=17{\cdot}2810$, and therefore the eccentricity (in our sense) is $e=0{\cdot}9672$, a figure which does in fact lie between the osculating values in 1607 and 1682. Changes in the inclination and in the positions of the node and perihelion are unimportant because the observations of Apian in 1531 and Kepler in 1607 are too rough for any certainty, but the effect of perturbations is pointed out. Halley could not calculate the effect of perturbations on the period with any accuracy, but he estimated the influence of Jupiter fairly shrewdly. For he predicts the next return "circa finem anni 1758 vel initium proximi", and in the circumstances this is a very good approach to March 1759, the actual date.

It is only in this second version (1752) that two noteworthy passages occur. The first, which is famous, runs:

"Quocirca si secundum prædicta nostra redierit iterum circa annum 1758, hoc primum ab Homine Anglo inventum fuisse non inficiabitur æqua Posteritas".

The second is a challenge, resounding at the time† when it was written:

"Experiantur itaq., *Vorticum et Pleni absoluti* fautores, an juxta Hypotheses suas poterint hujus Cometæ, per novem integra Signa ac spatium plusquam quadrimestre visi, cursum repræsentare; et an alia Curva, aliave in eâ motus lex a nostra sensibiliter diversa, singularem Viæ ejus Curvaturam, ac Velocitates diversimode auctas ac minutas pari certitudine poterit exhibere. Si hoc fieri nequeat, discant tandem missis nugis Veritatis studio indulgere, et cum Regali Societate nostrâ *Nullius in verba* jurare."

The date predicted by Halley for the first return of the Comet after 1682 was necessarily vague. If perihelion passage about mid-December 1758 be inferred from his language, that would be about three months too early. Since he considered only the effect of Jupiter, and had no means of calculating these perturbations accurately, the result must be considered quite a good one. The subsequent history of the Comet affords an excellent illustration of the development of methods for calculating special perturbations with increasing accuracy.

The calculation of the first return in 1759 was not left to the rough prediction of Halley. In the rising school of French mathematicians who were destined to exploit the work of Newton, Clairaut (1713–1765) was the first to distinguish himself by researches on the figure of the earth, planetary perturbations and the lunar theory. On the instigation of Lalande he undertook to calculate the perturbations of the Comet, making it a condition that he should have the help of Mme. Lepaute (née de la Brière) as computer. Clairaut devised the methods to be used in the then novel task, and with this aid finished the necessary calculations in eighteen months, so that he could announce the result before the end of 1758. The predicted time of perihelion was April 13 in the following year, thus overshooting the mark by a whole month, for the actual date was March 12. There was nothing surprising in the discrepancy, because the masses of the planets were very imperfectly known. An error in the assumed mass of Saturn was chiefly responsible, and neither Uranus nor Neptune was so much as known to exist. The outcome of the pioneer research of this kind must be considered entirely successful.

Once more the Comet has started on its long journey. Before it returns, mathematical astronomy will have made great strides. Euler will not see it again: he is too nearly the contemporary of Clairaut. Neither will Lagrange, nor Laplace. But all these and others will have enriched science by their

* "Newton ne nous a donné que des solutions imparfaites sur les comètes" (Lagrange).

† "The Synopsis of the Astronomy of Comets was printed off at the same time as the Tables" (Preface). This presumably means 1719, and the date of the manuscript was not later than 1717.

immortal labours, and there are many to hand on the torch. No fewer than four astronomers undertook the task of calculating the perturbations on this occasion, of whom one carried them back as far as 1607. For the date of perihelion in 1835 the results were: Damoiseau, November 4; Rosenberger, November 11; Pontécoulant, November 13; Lehmann, November 26. This time, thanks to the last shot (which is the most discordant), the target is bracketed. For the date actually assigned after the event was November 15, 1835. Rosenberger's seems to have been the most careful investigation entering into competition, and his predicted date is four days too early. It will be noted that since the last return Uranus has been added to the known list of planets, but Neptune still awaits discovery.

Before the Comet returns again, astronomy has enjoyed another period of orderly development. Thanks to Adams and Leverrier, Neptune has been included in the number of the planets, and under photographic scrutiny it seems unlikely that any other of comparable mass remains to be found. Thanks again to Leverrier and later to Newcomb, the masses of all the planets are now known with greater accuracy than before. A technical advance has been made in presenting the tabular co-ordinates of the disturbing bodies and the principles being recognized, in the calculation of special perturbations there is room for little more than a judicious choice in the management of the details. Thus the conditions for a close prediction in 1910 were favourable.

It is tempting to make one remark. The methods of science, once devised, cannot be controlled in their application. Thus the method of special perturbations, which follows the disturbed body in its motion step by step, is a form of interpolation, a method of which Newton himself was a master. But, being quite general in its character, it is essentially the same method as that employed in calculating the trajectory of a shell by small arcs, and hence the range table of a gun. It is therefore the same mathematical process which predicts the return of a comet or the course of a minor planet for the peaceful astronomer as that which gives land or naval artillery its deadly effect. This is an illustration of the simple and inescapable fact that science in itself is amoral.

In spite of the technical simplifications of which the process is capable, the task of calculating special perturbations over the three-quarters of a century (or rather over a century and a half, for two circuits of the course before 1910 were in fact followed, to ensure accuracy) is still laborious in the extreme. In this case the Lindemann Prize offered by the Astronomische Gesellschaft attracted only two competing essays, and there was no difficulty in deciding between them since one proved to assign a date two months later than the time of perihelion deduced from observation.

The successful essay was the joint work of Messrs. Cowell and Crommelin, assisted by several volunteer computers. Their date for the perihelion passage in 1910 was April 16·6. The comet was first recognized on a photograph by Dr. Max Wolf on September 11, 1909, and on March 5, 1910, the two English astronomers felt justified in stating:

"It now appears from the observations that the predicted time [April 16·61] is 3·03 days too early. At least two days of this error must be attributed to causes other than errors of calculation or errors in the adopted positions and masses of the planets."

This very precise statement indicates that the problem of Halley's Comet has not been completely solved and ensures that the return in 1986 will receive careful attention. As in 1835 according to Rosenberger, so in 1910 the Comet reached its closest approach to the sun too late to agree with theory. As all questions of the physical nature of comets are now left to be discussed by some future Halley lecturer, this is one which needs more evidence before it can be discussed profitably. The discrepancy is of the wrong sign to be explained by the resisting medium invoked to account for the anomalous motion of Encke's Comet. On the other hand, while motions in almost parabolic orbits have had their value for the theory of gravitation, the constitution of a comet is far removed from the ideal particle which the mathematician would choose as a test. The nucleus of Halley's Comet transited the sun on May 18, 1910. By no means whatever could any certain effect be observed. Similarly, in the middle of May the earth may have passed through the tail of the comet. Whether it did so or not, no effect was to be anticipated, and certainly none was seen.

From the moment that the periodic orbit of the Comet is assured, it becomes possible to trace its course forward or backward with equal facility—or the same irksome toil. Now bright comets have appeared in the past and not a few of them have gained historical notoriety. It is therefore a question of great interest to identify such comets when possible with past appearances of Halley's Comet.

Halley himself was the first to make the attempt. The comets of 1531, 1607 and 1682 gave him intervals of 76 and 75 years. On this evidence alone a great comet should have appeared about the year 1456. It happens that a comet seen that summer, when the Turks were threatening to overrun Europe, is one of the most famous historically in the whole series. The identification in this case has proved sound. But the mean period over two milleniums is not far short of seventy-seven years, and therefore when Halley tried to go back further (to 1380 and 1305) he made errors of two and four years, and would have been led further astray if he had not stopped at that point. The method was too crude for anything more than a short-dated suggestion. The date 1456 was confirmed by Pingré (1711–1796) on the more legitimate evidence of an orbit deduced from observations.

The next identification was made by Burckhardt (1773–1825) with a comet observed in 989. Though recorded in the Saxon Chronicles, it was also observed in China, and the Chinese observations made it possible to determine an orbit.

This recognition of an isolated return illustrates the fact that the most trustworthy data before the fifteenth century are to be found in the Chinese annals. Three appearances of Halley's Comet were thus identified by Laugier (1812–1872), an astronomer of the Paris Observatory. These occurred in 451 (coinciding with the Battle of Chalons), 760 and 1378.

But the most extensive research of this kind was made by J. R. Hind (1823–1895), who, like Dr. Cowell, was later to become superintendent of the "Nautical Almanac". His work covered all the appearances of the present era from 12 B.C. onwards, and though three or four have not been substantiated, a round dozen of true identifications remain to his credit. Where he failed, the cause is simple: the Chinese observations were insufficient to establish the orbit with the necessary precision. When the

Chinese record of the Comet's return gave adequate detail, Hind was right, and Dr. Crommelin has testified to the value of his work as a foundation for subsequent research.

Thus a few doubtful cases remained, as Hind himself indicated, and to decide them a much more laborious method was required. It was necessary to trace the Comet back through the centuries by tracing its perturbations continuously. This looked like a Herculean task, and it was in fact arduous enough. But Dr. Cowell devised a notable simplification in the method and the late Dr. Crommelin and his collaborators carried out the work. Not all the refinements necessary when prediction within a few hours is aimed at are needed in this backward sweep, but no means for securing essential accuracy were neglected.

RETURNS TO PERIHELION

(Cowell and Crommelin)

240	(B.C.)	May 15 ?	912	(A.D.)	July 19
163	,,	May 20 ?	989	,,	Sept. 2 (Burckhardt)
87	,,	Aug. 15	1066	,,	Mar. 25 (Hind)
12	,,	Oct. 8 (Hind)	1145	,,	April 19 (Hind)
66	(A.D.)	Jan. 26 (Hind)	1222	,,	Sept. 10
141	,,	Mar. 25 (Hind)	1301	,,	Oct. 23 (Hind)
218	,,	April 6 (Hind)	1378	,,	Nov. 8 (Laugier)
295	,,	April 7 (Hind)	1456	,,	June 2 (Halley, Pingré)
374	,,	Feb. 13 (Hind)	1531	,,	Aug. 25
451	,,	July 3 (Laugier)	1607	,,	Oct. 26 } Halley
530	,,	Nov. 15 (Hind)	1682	,,	Sept. 14
607	,,	Mar. 26	1759	,,	Mar. 12
684	,,	Nov. 26 (Hind)	1835	,,	Nov. 15
760	,,	June 10 (Laugier)	1910	,,	April 19
837	,,	Feb. 25 (Hind ?)	1986	,,	Feb. ?

The result is the accompanying table, which records the times of perihelion passage over a period of two thousand years, the outcome of an investigation which is a model of its kind. With the dates are given the names of the astronomers who first identified the return with a recorded comet.

Before A.D. 1400 the identifications rest mainly on Chinese observations, as already remarked. That does not mean, of course, that the Comet was not mentioned in European annals, but merely that the European descriptions have little astronomical value. On the contrary, the Comet has blazed its trail through the pages of Western history, as in 1066, and the historian may find great interest in a list of dates which, thanks to the labours of Cowell and Crommelin, he may accept as perfectly trustworthy.

When the central problem of astronomy is considered, and the importance of Halley's Comet in relation to that problem, one thing must be understood. The history of science is no mere catalogue of ideas and discoveries. It is not enough to separate the true from the false so far as that can be done at a given time, but it is also necessary to see how the truth came to be established and how error was finally overcome. In general, the process has been slower than is commonly realized. Even in our time, when development is continuous and is watched by a thousand eyes, we have seen eminent chemists reluctant to abandon their indestructible atom and accept the electron theory of matter. It is altogether wholesome that novel assertions should meet with critical reserve.

Very different was it in the ages when science progressed by fits and starts. Thus Aristarchus of Samos, the contemporary of Archimedes, proclaimed the heliocentric theory of the solar system. Eighteen centuries elapsed before the truth of the idea was recognized by Copernicus. It is sometimes implied that this was an independent discovery. But the origin of the idea was immediately pointed out by commentators in England, and it could not have escaped Copernicus himself, who had studied in Italy and elsewhere, and was a quite exceptionally well-educated man. His merit consists in reconciling the idea with the Ptolemaic theory. But as there was nothing to prevent anyone from doing the same thing at any time during the preceding fourteen centuries, he must be held to have written a belated last chapter of the old astronomy, not a first chapter of the new. Apart from what was due to Aristarchus, the errors of Ptolemy were perpetuated.

The new order in astronomy came in 1609 with the "Astronomia Nova" of Kepler. It was the result of studying new, prolonged and more accurate observations. For the first time in a work of this kind, the proportion of truth is greater than the proportion of error. It gave a sound foundation for further progress. But how slowly was its true character recognized, even in an age which was beginning to move rapidly.

There was one notable, though unproductive, exception. It is shown in that remarkable letter addressed by Sir William Löwer from the neighbourhood of Caermarthen to Thomas Harriot at Sion House. A few sentences from it may be given :

"February 6, 1610.

"I have received the perspective Cylinder that you promised me. . . . According as you wished I have observed the Mone in all his changes. . . .

"Kepler I read diligentlie. . . . Not of his causes for I cannot phansie those magnetical natures, but about his theorie which me thinks (although I cannot yet overmaster manie of his particulars) he establisheth soundlie and as you say overthrows the circular Astronomie.

"Do you not here startle, to see every day some of your inventions taken from you ; for I remember long since you told me as much, that the motions of the planets were not perfect circles. . . .

"But again to Kepler I have read him twice over cursoridlie, I read him now with Calculation. Sometimes I find a difference of minutes. . . . For this theorie I am much in love with these particulars.

"1° his permutation of the medial to the apparent motions, for it is more rational that all dimensions as of Eccentricities, apogacies, etc. . . . should depend rather of the habitude of the sun, than to the imaginarie circle of orbis annuus.

"2°. His elliptical iter planetarum, for methinks it shews a Way to the solving of the unknown walks of comets. For as his Ellipsis in the Earth's motion is more a circle* and in Mars is more longe and some of the other planets may be longer againe so in thos comets that are appeard fixed the ellipsis may be neere a right line.

"3°. His phansie of ecliptica media or his via regia of the sun, even the ecliptica vera under w^{ch} the earth walkes his yeares journie ; by w^{ch} he solves handsomelie the mutation of the starres latitudes. Indeed I am much delighted with his booke, but he is so tough in manie places as I cannot bite him. . . .

"... Above al things take care of your health, keepe correspondence with Kepler and whereinsoever you can have use of me, require it with all libertie. . . .

"WILLM. LÖWER."

* What precedes this point is from a paper at Petworth House, found and reproduced by the Baron von Zach. The continuation is in the British Museum. The whole was reconstituted by Rigaud and printed in the supplement to Bradley's works. It is also to be found in Henry Stevens's "Thomas Hariot, Mathematician, Philosopher and Scholar" (London, 1904).

Three things are notable in this letter of a Welsh squire, drawing his inspiration from Harriot. First, there is the early specific mention of the use of a telescope in England. Then there is the suggestion as to the nature of cometary orbits, which is in line with, and much earlier than, Borelli's. It is to be remembered that both Harriot and Lŏwer observed Halley's Comet in 1607. Finally, the appreciation of Kepler and the implied criticism of Copernicus are extraordinarily acute. Such insight is nowhere to be found outside the circle of Harriot.

Why the message of Kepler fell on deaf ears elsewhere is an interesting problem. But two of the evil influences can be seen. The first—and this will surprise many who have imbibed a legend without due qualification—was Copernicus and his school. This great man had served as a useful link with the past. From the beginning of the following century the effect of his doctrines was simply mischievous. This remark can be easily illustrated.

When Horrocks started on his brief career as an observer, he sought the advice of Gellibrand, the Gresham professor of astronomy. On this recommendation he procured the works, including the Tables, of Lansberg. Comparison with his own observations soon convinced him that Lansberg was wrong in theory, wrong in everything. His denunciation of Lansberg was unsparing. Then he turned to Kepler, and in spite of the admitted faults of the Rudolfine Tables quickly came to the conclusion that he who has Kepler has everything. The tables were based on faulty numbers and these could be corrected by his own observations. But Lansberg's, being based on faulty theory, were hopelessly wrong. Now Lansberg was just an orthodox Copernican living in the wrong century; the real cause which excited the wrath and contempt of Horrocks was the Copernican doctrine, which had grown completely out of date. The work in which he champions Kepler was published thirty years after it was written, and it may have failed to have its due effect. But it is significant that all those discussions of Keplerian problems between Hooke, Wren and Halley which preceded the composition of the "Principia" were subsequent to the year 1672, when the "Opera Posthuma" of Horrocks was at last published by Wallis and warmly commended by James Gregory and by Newton.

Meanwhile Galileo had lived and died, and was destined, thanks to a few simple experiments in dynamics, to transform the thought and the physical condition of mankind. But his attitude to the problem of planetary motion is generally misrepresented. Like Harriot, he received a copy of Kepler's "Astronomia Nova", but there is not the slightest sign that he ever read it. He was not an ardent mathematician and without a Harriot at his back he was less likely to 'bite' such a book than our worthy Lŏwer. It is equally unlikely that he had any taste for the Ptolemaic details of Copernicus. His heliocentric theory was the simple doctrine of Aristarchus, founded on the argument from analogy which he drew from the Jovian system of satellites. As such it would be noticed as the view of a remarkable man of that age, but nothing more. That it has come to be otherwise regarded is due to quite adventitious causes, which have at the same time floated Copernicus on a tide of popular fame. These causes are the events of 1616 in the life of Galileo, the placing of the "De Revolutionibus" on the Index in the same year, and the subsequent persecution of Galileo in his declining years. But for these events, which have tended to overshadow the infinitely greater merits of Kepler, it is extremely probable that the general estimate placed on Copernicus would be slightly different from what it is to-day. Even at the time, the incidents did nothing to promote sound scientific opinion.

Altogether, astronomical opinion in the first half of the seventeenth century was in a state of great confusion. The Copernican theory still had its ardent supporters, but its radical unsoundness became more and more apparent. Instead of stimulating fresh efforts to find a better theory and to examine Kepler, as it did in the end with Horrocks, the failure induced a general feeling of futility and despair. The true system of astronomy seemed to be unattainable. On the other hand, those who had persecuted Galileo came to see that they had won a Pyrrhic victory and were ready to tolerate any theory so long as it was not called Copernican. The ground was therefore ready for a new crop, and while the good seed of Kepler was only waiting to be planted and nurtured by the hand of the mathematician, the second evil influence appeared and prepared a harvest which may have served to supersede the lingering remnants of Copernicanism, its chief recommendation in many eyes, but otherwise was worse than useless. This was the Cartesian system of philosophy.

René Des Cartes (1596–1650) spent the last twenty years or more of his life in Holland, latterly in Leyden. His chief works were two, the "Discours de la Méthode" (1637–8) and the "Principia Philosophiæ" (1644). The solid reputation of the author rests on one of three appendixes to the first. This contains his work on geometry, wherein co-ordinates are treated by algebraic methods; this is original and it marks an epoch in mathematics. Another is a treatise on optics and contains Snell's law of refraction, until then unpublished but certainly known sixteen years earlier. The science of optics, physical optics in particular, was then in a primitive state and Des Cartes did little to improve it. His later work contains his ideas on the laws of motion and his theory of vortices as a cosmic principle. To put it bluntly, this theory is trash, and it is hard to treat seriously a philosophy from which it sprang. Yet in its day, which lasted for a century, it enjoyed extraordinary popularity not only among the ignorant but also among those who would naturally be assumed to know better. Huygens, the greatest physicist of the seventeenth century, Newton not excepted, was a typical and at the same time an exceptional case. Des Cartes was an intimate personal friend of his father, Constantijn Huygens, and Christiaan was nurtured on the pure Cartesian doctrine. He remained a Cartesian through life, though this did not prevent him from correcting Des Cartes on the laws of motion or developing his own science in his own original way. But the Cartesian prejudices remained with him to the end. In England the bigger men seem to have escaped the influence tolerably well, though there was a moment when, according to Whiston, the doctrine had gained a firm hold on the teaching at Cambridge. In France the attack was most severe and lasted well into the eighteenth century in academic circles. The reason is easily conjectured. The doctrine of vortices was designed as an escape from Copernicanism to satisfy orthodox Catholic prejudices, and was therefore popular in the France of Louis XIV; besides, it was the work of a Frenchman. This explanation does not apply to the special

case of Huygens, who was a Dutch Protestant, and of course the case of England is quite different. Another illustration of this line of thought is to be seen in the readiness of Voltaire to follow Newton.

It is sometimes too readily supposed that Newton's "Principia" met with immediate acceptance. Newton's reputation as a mathematician was great and had spread far beyond the limits of England. The book was recognized as a remarkable work in the limited circle of men who could read it intelligently. But if anyone thinks that its doctrines and its spirit was accepted even by the most competent judges of the time, he should read the letters which passed between Leibniz and Huygens. A few specimens may be quoted.

The first extract shows the eagerness of Huygens to see the "Principia", though he cannot conceal his Cartesian predilections. In the second, Huygens gives Leibniz excellent advice and seems to show signs of conversion; "besides, you know, these vortices are not without their difficulties". Then in the third he swings completely round: "Newton's principle of attraction seems to me absurd". Finally there is a fair sample of the obstinacy with which Leibniz clings to his Cartesian ideas. Let it be observed that, throughout, neither Huygens, the physicist who had first seen Saturn's ring with an understanding eye, nor Leibniz, the mathematician and philosopher, betrays the slightest sense of the needs of practical astronomy. The astronomer wants the "Nautical Almanac", and naturally has little patience with the philosopher who is so imprudent as to stand in the way of that legitimate object.

(*a*) Huygens to Fatio de Duillier (extract).

1687, July 1/11.

"Je souhaite de voir le livre de Newton. Je veux bien qu'il ne soit pas Cartesien pourveu qu'il ne nous fasse pas de suppositions comme celle de l'attraction.

"Cela me paroit assez étrange que les rayons employent un temps egal et qu'il ait pourtant un autre principe. La proposition du Globe assez paradoxe j'en voudrois voir la demonstration. . . .

"Mes respects a M. Boyle. Ayons le livre de Newton". ("Œuvres Complètes de Christiaan Huygens", **9**, 190.)

(*b*) Huygens to Leibniz (extract).

Jan. 30.
1690, Feb. 9.

". . . Je vois que vous vous estes encore rencontré avec luy en ce qui regarde la cause naturelle des chemins Elliptiques des Planetes mais comme en traitant cette matière vous n'aviez encore vû qu'un extrait de son livre et non pas le livre mesme, je voudrois bien sçavoir si du depuis vous n'avez rien changé à vostre Theorie, parce que vous y faites entrer les Tourbillons de Mr. des Cartes, qui à mon avis sont superflus, si on admet le Systeme de Mr. Newton où le mouvement des Planetes s'explique par la pesanteur vers le Soleil et la *vis centrifuga*, qui se contrebalancent. Outre que ces Tourbillons Cartesiens faisoient naitre plusieurs difficultés, comme vous verrez par mes remarques et mesme sans elles vous ne pouviez pas l'ignorer. . . ." (Œuvres Complètes, **9**, 366.)

(*c*) Huygens to Leibniz (extract).

1690, Nov. 8/18.

"Pour ce qui est de la Cause du Reflus que donne M. Newton, je ne m'en contente nullement, ni de toutes ses autres Theories qu'il bastit sur son Principe d'attraction, qui me paroit absurde, ainsi que je l'ay desia temoigné dans l'Addition au Discours de la Pesanteur. Et je me suis souvent etonné, comment il s'est pu donner la peine de faire tant de recherches et de calculs difficiles, qui n'ont pour fondement que ce mesme principe. Je m'accomode beaucoup mieux de son Explication des Cometes et de leur queues; et quoyque la chose ne soit pas sans cette grande difficultè, que vous remarquez fort bien, je ne trouve encore rien de meilleur que ce qu'il en dit, qui vaut mieux incomparablement, que ce qu'en a imaginé des Cartes. . . ." (Œuvres Complètes, **9**, 536.)

(*d*) Leibniz to Huygens (extract).

1692, Sept. 16/26.

". . . Il faudra examiner quelle explication est la meilleure, ou si on les peut concilier. Le même se peut dire à l'égard de l'explication de Mons. Neuton des ellipses. Les Planetes se meuvent comme s'il n'y avoit qu'un mouvement de trajection ou de propre direction joint à la pesanteur à ce que Mons. Neuton a remarqué. Cependant ils se meuvent aussi, tout comme s'ils estoient deferés tranquillement par une matière dont la circulation y soit harmonique; et il semble qu'il y a une conspiration de cette circulation avec la propre direction de la Planete. Et la raison qui fait que je ne me repens pas encor de la matière deferente, depuis que j'ay appris l'explication de Mr. Neuton, est entre autres, que je voy toutes les Planetes aller à peu près d'un même costé, et dans une même region, ce qui se remarque encor à l'égard des petites Planetes de Jupiter et de Saturne. Au lieu que sans la matière deferante commune rien n'empescheroit les Planetes d'aller en tous sens. . . ."

(Œuvres Complètes, **10**, 316.)

The views of Locke and Leibniz make an interesting contrast. Thus Locke:

"It is true, I say, 'that bodies operate by impulse, and nothing else.' And so I thought when I writ it, and can yet conceive no other way of their operation. But I am since convinced by the judicious Mr. Newton's incomparable book, that it is too bold a presumption to limit God's power, in this point, by my narrow conceptions. The gravitation of matter towards matter, by ways inconceivable to me, is not only a demonstration that God can, if he pleases, put into bodies powers and ways of operation, above what can be derived from our idea of body or can be explained by what we know of matter, but also an unquestionable and everywhere visible instance, that he has done so." ("Essay", p. 408, 1699.)

Locke also remarked that nothing is more likely to assist the sceptics than to deny what we do not understand. In his "New Essays" (1700, pub. 1765) Leibniz replies: "I fully recognize that it is not allowable to deny what we do not understand, but I add that we have the right to deny (in the order of nature at least) what is absolutely unintelligible and inexplicable."

After that exhibition of considerable egotism, is it strange that science should find in seventeenth century philosophy, the philosophy of Des Cartes, Malebranche, Spinoza and Leibniz, with all their differences, nothing much better than an obstructive irrelevancy? That is what Newton found it in the criticism of his theory of colours, and it is to be seen

again in the reception accorded to the "Principia". Though the fact has been overlooked, the feeling engendered at the time will help to explain the venom which entered into the controversy over the development of the infinitesimal calculus. In his last shaft, Leibniz virtually accuses Newton of atheism, than which at the time no insinuation could be more injurious. In liberality (and perhaps in clearness) of thought the lapse of a century shows remarkably little advance on the Inquisition which condemned Galileo.

Now all this seems very far removed from the story of Halley's Comet. The object has been to show by examples drawn from the history of astronomy that the acceptance of new and sound ideas is in general slow, and that the reception of Newton's "Principia" was no exception to the rule. It remains to be seen what bearing Halley's work on comets had on the outcome.

It has already been shown, and it is generally appreciated, that Halley's share in the production of the "Principia" was a considerable and quite possibly a vital one. In the first place, he directed Newton's attention with persuasive urgency to the great problem of astronomy. But when Newton's interest had been definitely engaged in the desired direction, circumstances arose which in a man of such delicate susceptibilities might easily have turned him from the completion of his task. Halley intervened and smoothed away the difficulties as they arose. At last the work was finished and safely delivered to the Royal Society. The Society was willing and eager to publish it, but at the moment it lacked the means. Halley, then a poor man, came forward and undertook the financial responsibility. But Halley also rendered a fourth service, and the nature of this is still to be seen.

The miracle of the "Principia" lies in its comprehensiveness. At the beginning Newton had perhaps a few fundamental theories, but even of these it was necessary to recover the proofs. From this point, within a period of barely two years, he has covered the whole field of gravitational astronomy, and that in considerable detail. The first steps have been taken in the lunar theory, the most difficult part and the one which ultimately supplies the most delicate test of the whole. Nutation* has been discussed, even before its discovery from observations by Bradley. The theory of tides has been developed; the attitude of Huygens to it has been noticed incidentally. A later event may also be noted. In 1740 the Paris Academy of Sciences proposed the subject of tides for competition. The three memoirs submitted respectively by Euler, by Maclaurin and by Daniel Bernoulli are all based on Newtonian principles. Yet even at so late a date as this there was a fourth which still maintained the Cartesian doctrine and commanded respect. The pleasantest aspect of this affair was the contribution of Daniel Bernoulli, for he was the son of John who, during the controversy over the "Commercium Epistolicum" was currying favour with Newton and at the same time stabbling him in the back, and the nephew of James, who in spite of his great gifts had strange ideas about comets.

But in spite of all those things in the "Principia" which excite wonder to-day, recognition of its fundamental truth was slow in coming. Nor is this altogether surprising, because it had to wait for that confirmation which rests on the comparison of its rapidly sketched theory with the result of accurate observations. This ultimate test came in time, and in spite of small discrepancies still outstanding, the main position is no longer in dispute. But this is a result of slow growth. First Bradley introduced a new order of accuracy into the observations. Then the theory of the planets was developed. But even after a century or more, even to-day, though the margin of uncertainty grows smaller, the normal difference between observation and calculation can rarely be traced to its source with perfect assurance. There is the error inherent in the observation. There is error in the system of numerical data adopted. There is possible error and always incompleteness in the practical development of the theory. There is the possibility of unknown and unsuspected disturbing influences of an alien kind. All these must be taken into account and assessed before judgment can be passed on the adequacy or failure of the general law of gravitation.

Therefore it is not surprising that at a time when the technical state of astronomy was still backward, the sceptics could find plausible excuses for their attitude. What was wanted to overcome the opposition of the Cassinis, who were practical astronomers, and the Bernoullis, who were not, was an example, preferably qualitative and if possible spectacular, to serve as an advertisement of the new system. By his "Synopsis Astronomiae Cometicæ" Halley supplied this want. Comets had always been unruly and erratic members among the heavenly bodies. In Kepler's day, though he understood them very imperfectly, they had destroyed the lingering remnants of the planetary spheres. By reducing them to order according to Newtonian principles Halley smashed the last traces of the Cartesian vortices, which had never explained anything because they were themselves without explanation. Hence the importance of the challenge to the Cartesians already quoted, which shows that he realized clearly the significance of what he had done, though these barbed words were only published thirty-five years after they were written. By that time the issue was no longer in doubt. But one thing remained to be seen, though not by Halley: the return of the famous Comet. It was first detected on Christmas Day, 1758, when approaching the sun. How it passed on in ts appointed track has been related. With it as it went once more into obscurity went also the last remnants† of that type of natural philosophy which is associated with the names of Des Cartes and Leibniz. In hastening that day, Halley rendered his fourth great service alike to Newton and to science.

NOTE

There is an interesting passage in Whiston's memoirs[5]:

"I proceed now in my own history. After I had taken Holy Orders, I returned to the College, and went on with my own Studies there, particularly in Mathematicks, and the *Cartesian* Philosophy; which was alone in Vogue with us at that Time. But it was not long before I, with immense Pains, but no Assistance, set myself with the utmost Zeal to the Study of Sir *Isaac Newton's* wonderful Discoveries in his *Philosophiæ Naturalis Principia Mathematica*, one or two of which Lectures I had heard him read in

* It must be allowed that Newton's nutation and Bradley's are not quite the same thing.

† Fontenelle, reputed the last Cartesian of any scientific standing, had died in 1757, thirty-three days short of his hundredth birthday.

the publick Schools, though I understood them not at all at that Time. . . . We at *Cambridge*, poor Wretches, were ignominiously studying the fictitious Hypotheses of the *Cartesian*, which Sir *Isaac Newton* had also himself done formerly, as I have heard him say. What the Occasion of Sir *Isaac Newton's* leaving the *Cartesian* Philosophy, and of discovering his amazing Theory of Gravity was, I have heard him long ago, soon after my first Acquaintance with him, which was 1694, thus relate, . . ."

Then follows an account of incidents during Newton's life at Woolsthorpe when he retired from Cambridge owing to the plague. Doubt has been cast on some details of this story. But it is corroborated in all essentials not only by Pemberton but also by an account left by Newton himself. There can be no doubt that it represents a story which Newton was rather fond of telling in his old age. If therefore any doubt is to be entertained as to its accuracy, that must be attributed to the failing memory of an old man. It does not seem impossible that some picturesque details have accrued in the course of years.

That is not the present point of interest. If according to this account Newton ever flirted with the Cartesian philosophy, that must have been during his undergraduate days. That at some time Newton must have studied "the fictitious Hypotheses of the Cartesian" is one of Whiston's essays in the obvious. As the protagonist of a system of natural philosophy which found its greatest obstacle in the hypotheses of Des Cartes, Newton could scarcely have failed to do so. But the attitude of complete antagonism was established at least as early as 1666 (*aet.* 23).

But Whiston was twenty-five years younger than Newton, whom he succeeded in the Lucasian chair at Cambridge in 1703. The year in which he took deacon's orders was 1693. Thus the years of which he is speaking, when the Cartesian philosophy was alone in vogue at Cambridge, was at least six years after the publication of the "Principia", and not very long before Newton left Cambridge for London. It appears then that whatever impression the "Principia" may have created outside Cambridge, inside Cambridge the teaching of Newton had been practically without any effect whatever.

At no time had Newton a gift for friendship. But that in itself is no reason why he should not have enjoyed the active support of discerning men. This he had, from Halley in the greatest measure, but also from others. David Gregory (nephew of James), the Savilian professor of astronomy, adopted the Newtonian philosophy from the first. When the controversy with Leibniz assumed an unpleasant aspect, it was to John Keill, another Savilian professor, that Newton turned as his champion. Locke wrote: "I have several reasons to think him truly my friend". Now all these were Oxford men. Even Henry Pemberton, the editor of the third edition of the "Principia", was introduced to Newton by Keill; he was not a Cambridge man. Robert Boyle, another acquaintance in correspondence with Newton, was a member of the group of men living in Oxford who founded the Royal Society.

It is inevitable that a comparison should be made with Newton's environment at Cambridge. If Newton sought no friends, it would still be supposed that some men of parts would seek his intellectual acquaintance. There is little evidence of it. Barrow resigned the Lucasian professorship in favour of Newton in 1669, became Master of Trinity in 1673 and died in 1677. That must have been a great loss to Newton. Whiston (of Clare Hall) made his acquaintance as related by himself, and Bentley cultivated it too, but philosophy rather than science was the common bond. This was only a few years before Newton left Cambridge; when Bentley became Master of Trinity (1700), Newton was established in London. But Bentley's account of the state of the College proves that Newton must have found it an utterly uncongenial environment. The impact of the Restoration on an academic community had been disastrous. At the same time Whiston's evidence goes to show that Newton was out of sympathy with the general trend of thought in the University of Cambridge at large.

These circumstances seem to explain much; why, for example, Newton was so ready to leave his quiet academic seclusion for the crowded life of the metropolis. It is just possible that Trinity was not so very quiet, while London and the Royal Society offered intellectual society far more to his taste. There is another aspect of the matter. In spite of Bentley's reforming zeal, which did produce fruit in a notable band of scholars, such as Porson, Cambridge in the eighteenth century, and Oxford too for that matter, failed conspicuously to take advantage of the opportunities opened up by Newton. An excuse has often been found in the notation (or absence of notation) of the fluxional calculus when contrasted with the methods of Leibniz. This seems hard to believe. Had there been a school of mathematicians, of the calibre of Colin Maclaurin for example, it is impossible to conceive that a mere question of notation would have been allowed to remain an obstacle when the successes of Clairaut and Euler were fairly considered. They did not receive this consideration because the men capable of giving it did not exist. The best English mathematicians of the period, like Simpson, Hutton and Peter Barlow, who were all connected with the Military Academy at Woolwich, owed nothing to university education.

[1] Cf. Zinner, E., "Leben und Wirken des Johannes Müller von Königsberg genannt Regiomontanus" (Munich, 1938).
[2] Pingré, "Annales Célestes du XVII^e Siècle", p. 359 (1901).
[3] Birch, "History of the Royal Society", 4 (1757).
[4] *Phil. Trans.*, No. 297, pp. 1882–99 (March, 1705).
[5] *Whiston's Memoirs*, 1, 35 (1749); Rouse Ball, "Essay on Principia" p. 8.

SOME BIOLOGICAL DISCOVERIES OF PRACTICAL IMPORTANCE*

By DR. C. H. WADDINGTON

THERE are two main spheres in which biology is of practical importance to society. One is medicine in the widest sense, which it would perhaps be more accurate to call human biology, and the other is food production or agriculture, also in the widest sense.

I do not propose to discuss the first of these fields in any detail, although new discoveries and new applications of old discoveries are always being made. Among recent new discoveries one may perhaps mention the bacteriocidal and bacteriostatic substances which are now being obtained from lower organisms. Penicillin, extracted from a mould

* Substance of a lecture delivered at the Royal Institution on February 12.

related to the ordinary bread mould, is being investigated at Oxford and elsewhere and seems likely to prove one of the most powerful aids in dealing with infected wounds, a matter of the greatest importance at the present time. It may, however, be rivalled by gramicidin, a substance prepared by American workers from certain types of bacteria which grow in soil; but perhaps the more optimistic view will prove justified and the two substances be found to attack rather different kinds of germs and thus to supplement each other. As an example of a new technique of utilizing old biological knowledge, one may mention the treatment of various diseases, chiefly cancer in some of its forms, by radioactive substances artificially prepared with the aid of new physical apparatus such as the cyclotron. Radio-phosphorus and radio-iodine are differentially absorbed by different tissues in the body; their radio-activity causes the destruction of the tissues in which they become located and the surgeon can in this way bring about a localized inhibition of particular tissues which are proliferating too rapidly.

But the great tasks of medicine in the immediate future are concerned with the application of knowledge which we already possess. These tasks can be summarized under three heads: first, the attainment in practice of optimal standards of nutrition, of housing, of industrial hygiene, and so on; secondly, the conquest of tropical diseases and the improvement of the health of tropical populations until they have an equal chance with the peoples of the temperate zones to play their part in world civilization; and thirdly, a task in which social biology must collaborate with political and other agencies, is the reversing of the general fall in the reproductive-rate in industrial countries. New discoveries may, and very probably will, be made which will assist in the carrying out of these tasks, but the main problems are certainly the administrative and political ones of applying existing knowledge.

When we turn to the field of food production we find a great body of existing knowledge which is in process of being put into practice. Such developments of farming practice as the system of ley-farming and the increased use of silage are examples. But there are also a number of biological discoveries which may have very revolutionary effects on the whole of our food production system, and it is primarily a summary of these more far-reaching possibilities that I propose to discuss.

We may mention first some new uses for old crops. Straw is bound to be produced in large quantities along with any cereal crop, but, except for some oat straws, it is itself of little value for feeding animals. It has recently been shown that a treatment with caustic alkali breaks down the hard tissues of the straw sufficiently to allow of its being digested by cattle, and a very considerable new source of feeding-stuffs has thus been placed at the farmer's disposal. A more striking innovation would be the direct use of grass protein for human consumption. Grass is much our biggest crop; we produce about 60 million tons a year. It is too full of indigestible fibre to be eaten by man in the raw state, and the methods by which we use it are still those which were invented in the Stone Age; we feed it to cattle and sheep and ourselves eat the beef, mutton, milk, etc., which they produce. They can, it is true, digest the fibres with which the human intestines cannot deal, but they are relatively inefficient in making use of the proteins. Now it is comparatively easy to grind up the grass and squeeze out some 30 per cent of the protein for our direct use. The remainder of the protein remains with the fibres, the whole mass still representing a better cattle food than ordinary hay. The technical problem of making the grass protein into a really attractive and palatable product should not long remain beyond the capacities of our biochemists. When it is pointed out that the total quantity of grass protein grown in Great Britain is something like ten times our normal protein requirements, the potential importance of the direct utilization of even a small fraction of it requires no further emphasis; but the economic and agricultural problems of employing the process on a large scale require further study.

Let us consider now some new technical possibilities in the cultivation of our crops. Much publicity has been given recently to tank-culture or hydroponics. By this is meant the cultivation of plants in culture solutions the concentration and composition of which can be accurately controlled. Such methods are, of course, by no means innovations in the laboratory; it is only their utilization on a large scale for commercially grown greenhouse crops which would be a new departure. It does not, however, seem by any means certain that this technique has any great advantages, at least in the climate of Great Britain, over the method of soil sterilization which was introduced some years ago and is now becoming very widespread.

Another technical innovation from which much can be expected is the use of the recently discovered plant hormones. Knowledge in this field is still growing rapidly but we already understand something of the part played in the growth of a plant by very small quantities of certain essential substances, some of which are in fact identical with the vitamins which we hear so much about in our own diet. Some of the hormones are already quite widely employed to encourage the rooting of cuttings; but there are many more possibilities emerging in the laboratories of the world—for example, the growing of long-fibred stems in textile plants such as flax and hemp, the production of seedless fruits from unpollinated flowers treated with suitable hormones, the control of the opening of flower buds until the danger of severe frost is past, and so on. Moreover, new hormones are still being discovered, some, such as a new pollen extract recently described by the United States Department of Agriculture, controlling growth, while others are concerned with various other phases of a plant's development.

These studies have not yet been brought into relation, although they soon must be, with another very important development which has arisen in practical crop management. Russian biologists in particular have devoted great attention to the investigation of the time of ripening of plants, with the view of developing types which can either survive their rigorous winter or which can crop in a single year after planting in the spring. They have developed a rather elaborate theory of the succession of various phases in a plant's development, which they distinguish sharply from its growth. In general a plant first passes through a temperature phase, in which it requires a certain length of exposure to particular low or high temperatures; this, they claim, is succeeded by a light phase, in which the plant must be subjected to particular conditions of long or short daily periods of illumination. The practical application and indeed the practical basis of this theory is

the technique known as vernalization, in which the germinated seeds are artificially given the treatment needed to complete the first phase of development before being sown. Thus a winter variety of wheat, which is normally sown in autumn and lies in the ground over winter, starting to grow in the following spring, may be artificially cooled for the required time and then planted in the spring to crop in the same year. Such methods would be of some value even in England and may be of very great importance in countries with the severe climatic conditions of Soviet Russia, where many million acres have been sown with vernalized seeds. Workers in other countries are not so convinced of the practical value of the method, and the details of both theory and practice are still under discussion. But there seems little doubt that the basic phenomena are quite real and that our understanding of the matter will rapidly become much more profound. It has already been shown that, in certain plants, substances capable of diffusing from a stock into a grafted scion are concerned in controlling whether flowering takes place in the first or second year, that is, with or without subjection to cold and short daylight, and it therefore seems most probable that the whole vernalization question will in time be brought into relation with the discovery of plant hormones already mentioned.

Some Soviet authors have claimed that a single vernalization treatment is sufficient to alter the hereditary constitution of the plants, so that its effects are permanent. Most biologists are likely to doubt the possibility of such a direct action, but it may well be that the Soviet workers have actually discovered a phenomenon of great importance. Since the development of a plant, as we have seen, is profoundly influenced by the early treatment of the seed, a single artificial treatment which causes the plant to mature at an abnormal season may have some effect on future generations owing to the altered conditions under which the seed will be formed. Similarly, another phenomenon on which the Russian scientific workers have laid great stress is the effect of a host plant on the hereditary constitution of a graft; again, the effect may not be a direct one on the hereditary elements as normally understood, but it may be a real phenomenon based on the transmission of something in the nature of a virus.

This brings us to the subject of plant-breeding, one of the fields in which the most spectacular advances are likely in the near future. The breeding of disease-resistant varieties, which, for example, trebled the yield of sugar cane in Louisiana between 1926 and 1929, will probably score some new successes but it cannot be considered a new technical advance. On the other hand, the practical employment of hybrid vigour is a development of the last few years in maize growing. At the present time something like twenty-five million acres are sown with hybrid corn in America, and increases in yield average about 20 per cent. Probably there are similar benefits to be obtained in other crops. The John Innes Horticultural Institution, for example, has prepared hybrid tomato seed with very considerably enhanced productivity.

The most important advances, however, are likely to come from two fields which are still being intensively investigated. The first is the study of the wealth of forms available in Nature for our use. Until recently the range of types employed by the plant breeder has been getting progressively narrower as he concentrated on the improvement of ever more specialized varieties. But it has been realized that much may be gained by the introduction of new hereditary material and it has come as something of a shock to discover how rich is the stock of forms available. Not only are there very numerous local varieties of cultivated plants, each probably with many defects by modern standards although with one or two points of advantage, but there is also a hitherto unexpected variety of wild forms related to the cultivated species. For example, all the cultivated potatoes of the Old World derive from a single species and probably from a very few individuals. We know now at least thirty species, forming a polyploid series, all of which were thought worthy of cultivation by some tribe in South America, and there are also related wild species. They include frost-resistant and short-day forms and varieties resistant to various diseases. It is not too much to hope that we shall fairly soon be able to develop potatoes suitable for the Arctic and for the tropics. The profound results of such a spread of the potato belt do not need to be emphasized.

The second major source of new varieties may be found in the very recent technique of artificially doubling the number of chromosomes in hybrids, thus rendering them fertile and comparatively stable in hereditary constitution. Nearly all our most important crop plants have arisen by a chance occurrence of a similar process in Nature. Now that we have discovered substances, the most important of which is the drug colchicine, which allow us to double chromosome numbers with fair regularity, it is almost inconceivable that we shall not be able to manufacture completely new types of plants of the greatest importance. It is too early as yet to name any examples with complete confidence, but the hybrids of wheat and rye, and of wheat and couch grass may be mentioned as instances of promising beginnings.

An advance of another kind is the recent success in hybridizing yeast. Until recently it was thought that if a single yeast cell was isolated it must give rise to a pure colony. But single yeast cells may be either haploid with one of each kind of chromosome or diploid with two. In the latter case haploid cells will be formed during the growth of the colony and the segregation of different types will occur, so that the final colony is mixed. Winge, in Denmark, has succeeded, by a fine technique of micro-manipulation, in isolating single haploid cells, hybridizing them and finally isolating the segregants from the hybrids. This is the first application of modern breeding methods to some of man's oldest plant collaborators, his assistants in the manufacture of such staples as bread and beer.

If we turn now to the animal field, many of the methods which seem to promise best among plants do not appear very feasible. Thus there are great technical difficulties in making such a full use of hybrid vigour, although some promising results have been obtained with fowls. Again, only the very first steps, in which I have myself shared, have been taken towards doubling the chromosome number in animals, and the difficulties to be surmounted before we can produce fertile hybrids in this way are rather overpowering in prospect.

Perhaps the most important new technique which is just passing from the laboratory into general practice is that of artificial insemination. If the

sperm of a selected male is artificially introduced into the female, the greater degree of control over the process makes it possible to employ much smaller quantities than Nature provides, and the number of offspring which can be obtained from a single male may therefore be multiplied some hundreds of times. This makes it possible to ensure that only the very best animals are used as fathers and the constitution of poor stock may thus be quite rapidly improved. The method has already been tested out on quite a wide scale and it has been found possible to send the sperm of desirable males for very considerable distances by air. The method has a very great part to play in the immediate post-war world when we are confronted with the problem of restoring the scorched earth of eastern Europe.

A development of the technique of artificial insemination which seems likely in the next ten or twenty years is the artificial determination of sex by the separation of male- and female-determining sperm. Such a possibility is, of course, a favourite theme of all kinds of charlatanry and psuedo-science. But some recent work, particularly in the U.S.S.R., indicates a serious possibility that the problem will be solved by the method of electrophoresis, that is to say the passage of an electric current through a suspension of sperm, causing an accumulation of the female-determining sperm at the anode and of male-determining at the cathode. There are still many technical details to be worked out, such as the most favourable salt solutions, temperature, etc., but it would be a bold man who denied the possibility of a fairly early solution of the problem. The importance of such a method for the dairy and poultry industries would be profound, but its application to human affairs obviously presents aspects of great difficulty.

It would be in the highest degree desirable to bring about a similar increase in the reproductive capacities of the females of domestic animals. This may be too difficult a task, but some increase can already be envisaged. The administration of suitable hormone preparations, for example, can bring a female into the breeding condition at a time of year when she would normally not be ready to receive the male. Some success has already been achieved in obtaining in this way two crops of lambs a year instead of one. Again, when a female is ready to breed the number of ova produced, and thus the number of young born, is rather restricted in many of our domestic animals, and treatments are being worked out which should make it possible to cause the formation of more ova. This will, in the first instance, enable us to cause a regular production of twins by our beasts. For particular purposes of breeding from exceptionally favourable females it may prove possible, by a technique which has already been used in rabbits, to increase the number of ova many times and to transplant some of them to the uteri of other females, which would then act as foster-mothers from the very earliest stages of the animal's development.

The control over developmental processes given us by the use of hormones may play a part in several other ways. We are beginning to have some understanding of the role of such substances in lactation. We know there are hormones which control the initial development of the mammary glands as well as the intensity of their secretion, and again it has proved possible to influence to a significant degree the content of milk in several important constituents. All these investigations, still essentially laboratory matters, are likely fairly soon to reach the point where we can begin to apply them in practice, when they may open up great possibilities of control over our milk supply, particularly in the difficult winter months.

A somewhat more speculative possibility is the employment of hormones for the control of size. In some animals at least, for example, the rat, it has been possible to obtain considerably increased growth by suitable hormone injections. Further, the embryo of a mammal is in a somewhat similar position as regards its mother as the plant grafts mentioned earlier; an effect exerted primarily on the mother may, by influencing the early development of the fœtus, produce an alteration which will affect the development of the next generation of young and thus be transmitted potentially for ever. That such an influence of the mother on her offspring of the first generation is a possibility is known from crosses between large and small breeds of horse, in which the size of the mother has a great effect on the size of the foal. An effect in more remote generations has been suggested by some workers using extracts of the thymus gland on rats. The matter is still very uncertain, but if it can be put on a firm basis very important results might be obtained. A further and still more speculative possibility may perhaps be worth mentioning; it has been claimed that the very early administration of growth-promoting extracts has a differential effect on those organs which develop most rapidly at early stages, particularly the brain. It is not clear whether the swollen-headed rats so produced were any cleverer than usual, but the possibility of such an effect may be worth considering, if only as a day-dream to solace the despair to which most educators are from time to time reduced. Let us hope, however, that man is already intelligent enough to use, for his benefit and not only for his destruction, the gifts which science offers, without waiting for a hormonally induced enlargement of his brain.

OBITUARIES

Sir Francis Younghusband, K.C.S.I., K.C.I.E.

SIR FRANCIS YOUNGHUSBAND, whose death occurred on July 31, was an outstanding example of that rather rare combination—a philosopher and a man of action. A passionate admirer of the beauties of Nature he had, almost from childhood, pondered and studied the mystery of living things—their origin, their evolution and their spiritual significance—and during his later years he embodied his thoughts and his philosophy in a number of books and papers.

His more practical activities as a man of action found their chief outlet shortly after he arrived in India as a young officer of cavalry. His first journey, in 1886, at the age of twenty-three, was made in company with Mr. H. E. M. James of the Bombay Civil Service, and Mr. H. Fulford of the Chinese Consular Service, when they travelled through large areas of Manchuria and explored in particular the range known as the Long White Mountain. An account of this journey was given by Mr. James in his book of that name, in which he pays tribute to the energy and ability of his young companion, who made himself invaluable in a variety of activities—surveying, the collection of entomological and botanical specimens, sketching, and so on. These wanderings

extended over a period of eight–nine months, and were immediately followed by Younghusband's famous journey across Central Asia from Peking to India, culminating in the crossing of the hitherto unknown Mustagh Pass into Baltistan. This journey is described in his own book "The Heart of a Continent", one of the classics of Asiatic (or indeed of any) travel, admirable alike both for the intrinsic interest of the journey itself and for the style in which the book is written. The publication of this book at once placed Younghusband in the foremost ranks of contemporary travellers and writers.

From this on his active life, both as an officer of the Indian Foreign and Political Department and as a traveller, continued almost without a pause until his retirement on pension in 1910. During the years 1888–91 he made a series of journeys through the wild areas of Central Asia, the Pamirs, Kashgaria, and certain hitherto unexplored tracts lying to the north of the Karakoram and Mustagh ranges. These journeys had a political as well as a geographical interest and importance. Later he served in Chitral, and travelled in South Africa as *The Times* correspondent just prior to the outbreak of the Boer War.

Younghusband was thus a well-known figure with a world-wide reputation when, early in 1903, he was appointed by the Viceroy, Lord Curzon, as commissioner in charge of the projected mission to Tibet, then in process of preparation on the Darjeeling frontier.

It was here that I first met him, and in the capacity of secretary and interpreter to the mission I was constantly by his side until he returned to India some sixteen months later on the conclusion of his task, and I was thus able to get to know him intimately and to appreciate his character and his capabilities. A man's character may often, I think, be estimated not so much from his conduct on the great occasions and crises of his life as from his reactions to the petty affairs of everyday and the small worries and mishaps inevitable during travel or a military expedition. Of these we had our full share during the mission—cold, fatigue, boredom, and at times danger and excitement. During all these critical months, when the chief responsibility for success or failure rested on his shoulders, I never once saw him, even for a moment, in the slightest degree flurried, impatient, cross, or in any way perturbed. He remained always cheerful and good-humoured, and apparently indifferent to the small matters which affect ordinary mortals. This was not a pose. It was an attitude of mind based upon his essential equanimity. He knew very well what he wanted and how he proposed to get it, and having thought all this out carefully he was able to relegate all minor considerations to their proper place. But at the same time when any major difficulty or danger, or difference of opinion arose which seemed likely to interfere with his plans or the progress of the mission, he was persistent, inflexible and courageous to a degree.

It was, in fact, greatly due to this combination of qualities—on one hand infinite patience where patience and conciliation appeared advisable, and on the other readiness to fight and to accept risks and responsibilities at critical moments—that our mission succeeded as it did.

It is an old story now and has often been told, but it should be remembered that it was not an easy business. The Tibetans were obstinate and pugnacious; and the dispatch of a mission with a small military escort into the depths of Asia, hundreds of miles beyond the Himalayas, and separated from its base by several lofty mountain passes and (when at Lhasa) by a great unbridged river, resembled more the adventures of Cortes or Pizarro than modern military and diplomatic procedure. There were occasions when failure seemed imminent and inevitable. But, as I have remarked above, it was the combined tact and firmness of the Commissioner which always overcame the crisis, and which eventually secured not merely a favourable treaty with the Tibetans but also the goodwill and friendship which have lasted from that day to this.

I have dwelt at some length on this Tibetan affair, partly because it is the culminating point in Younghusband's career as a man of action, and partly because it presented such an opportunity for the exercise of his peculiar qualities. He retired from the service a few years later, and during the remainder of his life devoted himself to work with the Royal Geographical Society, the organization of the Mount Everest expeditions, and to writing.

During these latter years he published a number of volumes dealing with his favourite subjects: the beauties of Nature, Indian aspirations, and philosophical musings and conclusions. Of these latter the volume which perhaps summarizes most fully the general trend of his mind is "The Living Universe", published in 1933. In this book he traces the genesis and development of this planet from fiery vapour to the present day, and he outlines his conclusions on the vexed and oft-debated question of the origin and evolution of life and intelligence from apparently dead matter. Space does not permit of any attempt to follow him in this thesis, but a perusal of the book will give a good general conception of his attitude towards such questions and the evolution of higher types of beings on this and possibly on other planets; and in some most interesting passages he reveals certain mystical experiences which have come to him.

It was during these latter years, too, that Younghusband organized the World Congress of Faiths, representing all the world's chief religions. The catholicity of his views, and the wide respect which these views commanded, were well illustrated in the memorial service held at St. Martin-in-the-Fields on August 10 when, besides the customary Christian prayers and hymns, extracts from the Hindu, Jewish, Buddhist and Moslem scriptures were read by representatives of each of these faiths.

Such a character is rare, and when his achievements as a soldier, explorer and diplomat are borne in mind, perhaps unique. His friends will always remember him with affection for his patience, tolerance, good humour, simplicity and utter sincerity.

FREDERICK O'CONNOR.

Mr. D. B. Mair

DAVID BEVERIDGE MAIR, who died at Galashiels on July 21, was well known as a mathematician and was a distinguished member of the Civil Service. He was born in Canada on March 24, 1868. His father, who was a Presbyterian minister, of an Aberdeen family, died young. David Mair was at school at Dollar Academy. At the age of sixteen he entered the University of Edinburgh and obtained first class honours in mathematics and in natural philosophy. He went to Cambridge as a scholar of Christ's, and was second wrangler in 1891. In 1893 he was awarded a

Smith's Prize and in 1894 was elected a fellow of Christ's.

During the next two years, part of Mair's time was spent in study abroad, particularly in Germany, and part in lecturing to honours students at University College, London, under Prof. Karl Pearson. In 1896 he was appointed a junior examiner in the Civil Service Commission; from that time, although he kept up his mathematical studies (including that of relativity), he devoted almost the whole of his energy to his official work. He had a powerful and original mind and would undoubtedly have done valuable work in mathematics in the greater freedom of an academic career. However, he found adequate scope for his abilities in the many problems arising out of the work of the Commission; he became head of the examination branch in 1907 and in 1918 was also appointed an assistant commissioner.

By nature Mair was an ardent reformer; but to the zeal of a missionary he added the remorseless logic of a mathematician and the open-mindedness of a scientific investigator. His prentice hand he tried on the mathematical question papers of school standard, eliminating book work and formal questions such as "solve the equation . . .", and substituting problems, particularly those of a practical nature on matters of everyday life. All candidates were required to use mathematical instruments and in some schemes of examination, for example, Army Entrance, laboratory tests in mathematics were added. For these tests he developed a fully equipped laboratory for the simultaneous examination of a number of candidates. As opportunity arose he endeavoured to get the syllabuses changed by removing the icy hand of Euclid and cutting out dead wood. At a later date somewhat similar treatment was accorded to the examinations of higher standard. There is little doubt that his efforts had a very considerable effect in improving mathematical teaching in Great Britain.

In course of time similar ideas were applied to other subjects, such as natural science, modern languages, etc. Nothing was too small to escape his eagle eye and fertile brain. For example, he devoted much time to drafting a syllabus in handwriting and devising a scheme by which a candidate could obtain full marks for a clear workaday hand, instead of spending laborious hours in order to produce in the examination room a beautiful specimen of what was known as the "Civil Service hand"; for spelling he evolved a type of question paper on a phonetic basis for which he designed some special symbols.

Naturally, he gave much attention to wider problems such as the drafting of schemes of examination and the adjustment of candidates' marks. As regards the latter subject, the methods used in the Commission were far in advance of those of other examining bodies; little information was published but particulars were readily given to inquirers.

In drafting schemes of examination, Mair endeavoured to bring them into close connexion with the educational system of the country, so that candidates could take the Civil Service examinations in their stride without special preparation at school or college, or recourse to a crammer. Besides the advantage to the candidate, this procedure widened the field and was thus of benefit to the State. He was also strongly in favour of selecting candidates of mature age by interview and record instead of by a written examination—the traditional Civil Service procedure. Further, he was always prepared to experiment with new methods, such as intelligence tests, to devise means of estimating their value, and to discard them if they proved useless.

A great impetus was given to the improvement of the examination methods of the department by the appointment of Mr. (later Sir) Stanley Leathes as first commissioner. He was a man of great ability and wide knowledge, an experienced examiner, and much interested in educational matters, particularly in the teaching of modern languages. In 1919 the Treasury set up a committee to consider the scheme of examination for Class I clerkships, the highest grade in the Civil Service, with Leathes as chairman and Mair as secretary. This Committee produced a very interesting report and recommended many important changes, including the introduction of a *viva voce* test and elementary papers in English, general knowledge, a foreign language and elementary science, in addition to advanced papers in the candidates' special subjects. The scheme, with certain variations, was adopted for the whole administrative group, namely, Class I clerkships, appointments in the Indian Civil Service, Foreign Office clerkships, etc.

During the War of 1914–18, Mair assisted Sir Alfred Ewing in the Intelligence Department of the Admiralty in deciphering intercepted German code messages. In 1921 he went to India to preside at the first examination held in that country for appointments to the Indian Civil Service.

Mair retired from the Civil Service in 1933 on reaching the age limit, and since then spent much time travelling abroad. He was a singularly modest man and never courted publicity. Small of stature but stoutly built, he was of impressive appearance; he had a piercing and very intelligent eye, a clear resonant voice, and a strong chin. He held decided views on most subjects and was always prepared to express them, and, if need be, to throw them overboard. He possessed a dry humour, well under control.

His publications included an elementary text-book on mathematics and one on fourfold geometry. He married in 1897 Jessy, daughter of Mr. William Philip. She survives him with a son and three daughters. Mrs. Mair was for many years secretary of the London School of Economics, and their eldest daughter is Dr. Lucy Mair, well known as an anthropologist.

LENNARD BLAIKIE.

WE regret to announce the following deaths:

Prof. Kurt Brandenburg, professor of special pathology and therapy in the University of Berlin, editor of the *Medizinische Klinik*, aged seventy-three.

Dr. W. A. Bryan, director of the Los Angeles Museum of History, Science and Art, on June 18, aged sixty-six.

Prof. A. Döderlein, formerly professor of gynæcology and obstetrics in the University of Munich, a pioneer in the bacteriology of the puerperium, aged eighty-one.

Dr. Laurence S. Moyer, of the Department of Botany, University of Minnesota, known for his work on the biochemistry and physiology of plants.

Prof. Francis Ramaley, emeritus professor of biology in the University of Colorado, botanical editor of *Ecology*, on June 10, aged seventy-one.

Sir Montagu Sharpe, K.C., the well-known ornithologist, chairman of the Council of the Royal Society for the Protection of Birds, on August 23, aged eighty-five.

NEWS and VIEWS

H.R.H. The Duke of Kent, K.G., P.C., K.T.

IT is with deep regret that we record the death of H.R.H. The Duke of Kent on active service in a flying-boat crash in Scotland on August 25. The Duke was thirty-nine years of age. For ten years, as Prince George, he served in the Royal Navy, and it was not until he was withdrawn from active service with the Navy that his talents and capabilities for public service could be thoroughly appreciated. As president of St. George's Hospital, he took a great personal interest in its welfare and prosperity. At the age of twenty-six he joined the Foreign Office and later moved to the Home Office, where he did important work in the inspection of factories and in other problems of sociological interest. His keenness for this work inspired him to sever his official connexion with the Home Office and to study industrial problems at first hand. His prolonged tour of the industrial north in 1931, followed by a series of less formal visits to factories and workshops, will long be remembered.

Sir Harold Scott, K.C.M.G.

SIR HAROLD SCOTT, who retires from the post of director of the Bureau of Hygiene and Tropical Diseases on August 31, has made notable additions to our knowledge of tropical medicine and comparative pathology. In Jamaica, when Government bacteriologist, he elucidated the problem of the so-called 'vomiting sickness', proving it to be due to poisoning by the ackee fruit (*Blighia sapida*), a common native food on the island, wholesome when ripe and naturally opened, but highly toxic in its unripe, unopened condition. He also investigated another baffling disease in Jamaica, showing it to be a 'central' and not a 'peripheral' neuritis as previously thought, but of toxic origin closely allied to certain avitaminoses occurring in Africa and elsewhere. As Government bacteriologist in Hong Kong he added much to the knowledge of tuberculosis by his investigation of its prevalence and character among Chinese of the poorer classes. Later, when pathologist to the Zoological Society of London, he was able to compare his three hundred postmortem studies of fatal human cases in Hong Kong with similar studies on wild animals dying of tuberculosis in the Zoological Gardens. His results, published by the Medical Research Council in 1929, throw much light on the pathology of the disease, the portals of entry, and on the lymphatic systems in relation to paths of infection within the body.

As medical secretary of the Colonial Medical Research Committee during 1928–30, and as assistant director and then director of the Bureau of Hygiene and Tropical Diseases, Sir Harold has greatly promoted and encouraged research in the Colonies. His "History of Tropical Medicine", published in 1939 and based on his Fitzpatrick Lectures delivered before the Royal College of Physicians, is a monument of learning and exposition.

Science and the War Effort

PROF. J. D. BERNAL, speaking on "Science and the War Effort" at a meeting called by the Staff Associations of the National Physical Laboratory and associated laboratories on July 28, referred to the changing role of science in warfare. The conception of the function of the scientific worker and technician at the beginning of this War, as during the War of 1914–18, was confined to dealing with such technical devices of war as were beyond the scope of Service personnel. It is, however, becoming increasingly realized that science interpenetrates war activities over the whole field, and the scientific method is becoming a general mode of approach to the whole field of military action. During the last 150 years, tradition has ruled warfare and tactical surprise has played a small part. Following the War of 1914–18 an attitude of complacency has ruled and we have not attempted to apply scientific thinking further to war problems. On the other hand, the Germans, having been defeated, have spent the intervening period in making a far-reaching and comprehensive study of the reasons underlying that defeat. It seems to be a general feature of history that a defeated nation intensifies its study of warfare, and produces surprising tactical changes. Thus the enemy has obtained an advantage in that he has built up a powerful war machine based on the scientific method. We have accordingly to set ourselves the task, not only to catch up with him, but also to surpass him quickly.

In setting ourselves to this task we have two advantages. In the first place we have not been subjected to the intensive concentration on war machines to the exclusion of other fields of scientific endeavour; and secondly, whilst German methods are undoubtedly effective, they do not lend themselves to elasticity of thought in new developments. It is here that our free and democratic methods might be turned to good account. Addressing himself to the problem of mobilizing science to speeding up the war effort, Prof. Bernal said we have to apply scientific resources to the War in a strategic way. Thus we must apply all available forces to definite ends. In the past our efforts have been somewhat unbalanced. The probable yield from any effort must become the criterion. Military affairs are such that, owing to the difficulty of changing over, unless, say, a 100 per cent yield is to be expected in a year, the work is not worth doing. There exist generally several restrictive features which must be removed if scientific work is to be fully utilized. Perhaps the greatest handicap is the lack of direct contact between scientific workers and their opposite numbers in the Services. This is often accentuated by excessive secrecy. Some bottle-necks are due to established departmental procedure and others are due to small departments which are trying to deal with a greatly increased volume of work with unexpanded staff and equipment.

The full effects of science on war cannot be felt unless the scientific mind can be brought to bear on the war as a whole. This is not only a matter for the 'higher ups' but every scientific worker has now a widened responsibility, and a revaluation of responsibility is required. This brought Prof. Bernal to his main point, that it is not sufficient merely to work harder than in peace-time. It is also necessary to examine the implications of the work and search for ways and means of expediting progress and improving organization. The person on the job is in a position where he or she can make useful suggestions in these directions, and the speed and efficiency with which we must work require of the person on the job that he does consider this to be a part of his responsibility. Here a special duty falls to the organized scientific workers, and it is now possible, without friction, to carry out the implications of this wider responsibility through joint consultation on matters affecting

efficiency. In conclusion, Prof. Bernal expressed the belief that the undifferentiated way of dealing with things which is characteristic of the scientific method, will, given good organization, build up a far better war machine than the one we have against us.

The War and Nesting Birds

THE most interesting effect of the War upon nesting birds in Great Britain has been the attraction of the black redstart (*Phœnicurus ochrurus gibraltariensis*) to nest in the air-raid ruins of some English towns. Until the War, this was one of the rarest British nesters, although a regular winter visitor, but on the Continent where it is a familiar town bird it shows a partiality for ruins and it would be interesting if it becomes a commoner British resident in this unexpected way. In 1923 this bird was first noted nesting in Great Britain in a southern county and in 1936 it began nesting in Cambridge. In 1940 and 1941 it nested in the city area of London and a considerable number of London Natural History Society observers watched the birds. Another influence upon nesting birds has been the military disturbance of many coastal and rural haunts, the felling of woods and the draining of marshland. Another rare British nester, the black-necked grebe (*Podiceps nigricollis nigricollis*), was under observation by the Merseyside Naturalists' Association this year at a new nesting site at Abbot's Moss, the third it has used in Cheshire. In Anglesey there was a considerable increase in the number of roseate terns nesting at Llandwyn island, where between sixty and seventy pairs nested this year with Sandwich terns, etc., and new terneries, possibly an overflow of this species, were made at Malldraeth Point and on Ynys Ferrig rocks in Rhosneigr Bay. The kite has, however, maintained its nesting haunt in the valleys of mid-Wales and the black guillemot its recent habitat on the Cumberland cliffs at St. Bees Head.

The British Association

THE report of the British Association for 1942 (Vol. 2, No. 6) is now available (price 5*s*.), and contains the full text of the papers given at the conference on European agriculture held by the Division for the Social and International Relations of Science in London during March 20-21, 1942. Problems in post-war reconstruction was the subject of discussion at the conference, the papers falling into four main sections: measures for reconstruction, economic and kindred problems, the future betterment of European farming and problems of peasant farming. The contributors, many of whom came from occupied countries and represented very different types of farming, were remarkably in agreement as to the causes of pre-war agricultural problems and the main lines upon which future reconstruction should be based, namely, the development of small-scale peasant farming, concentrating primarily on live-stock, fruit and vegetable production in order to raise the standard of living and nutrition.

In addition, the report contains an article by Sir Henry Lyons on the work of the International Research Council (now the International Council of Scientific Unions) which will be of special interest to those concerned with the possibility of increasing collaboration in science through some form of standing organization, and an account of the peace-time activities of the British Association by its president, Sir Richard Gregory.

The Study of Soils

THE Cantor lectures delivered at the Royal Society of Arts by Dr. B. A. Keen in January and February on soil physics, soil cultivation, and cultivation and crop yields have been published in the Society's *Journal* (**40**, 545; 1942). The study of soil physics is relatively new, and only within the last twenty or thirty years has this side of agricultural practice been placed on a sound theoretical basis. The substitution of the pore-space theory in place of the capillary-tube hypothesis as an explanation of the water relations within the soil has, for example, led indirectly to improvements in certain practices in irrigation areas and in land drainage. The effectiveness of different types of cultivation and the influence of weather on the formation of tilth can now be measured by studying the resulting soil structure, and both the design, performance and use of implements can accordingly be put on a more scientific basis. As regards the effect of cultivation on crop yield, the traditional views held in Great Britain are called in question. Experiments on certain soils have shown no special benefits to the crop from additional or increased depth of cultivation, and since cheap labour is no longer available and every mechanized operation costs fuel and oil, a plea is made for similar investigations to be carried out in widely different districts, so that reliable information of a more general nature on such an important economic question may be forthcoming.

Canadian Water Supplies

EIGHT years ago the Canadian Department of Mines and Reservoirs began a survey of all the water supplies in Canada, with the view of determining the mineral and other content of the industrial and domestic water of the Dominion. The final results of this survey are now published in "Industrial Waters of Canada", by H. A. Leverin (Ottawa: King's Printer. Pp. 112. 25 cents). The investigation involved the determination of suspended matter, silica, iron, calcium, magnesium, sulphate, alkalis, potassium, bicarbonate, free carbonic acid, chloride, nitrate, oxygen, hydrogen-ion and colour. The report discusses the origin and distribution of these impurities and their importance in many industrial processes. It appears that distilled water is necessary not only in the manufacture of chemicals or drugs but also in the manufacture of ice, storage batteries, yeast, aerated waters and the plating of mirrors. In the paper and pulp industry, in the making of such varied commodities as textiles, soap and ice, and in brewing, distilling, baking, tanning, canning, etc., the nature and amount of the impurities are of much significance. The processes used in the investigations are fully described, and the publication contains tabulated analyses of surface and civic water supplies and maps showing the hardness of town water supplies throughout the Dominion.

Tuberculosis in El Salvador

THE March issue of the *Boletin de la Oficina Sanitaria Panamericana* contains an interesting paper by Dr. Rafael Vega Gomez, head of the Tuberculosis Department and ex-director of health, El Salvador, on the control of tuberculosis in his country. The campaign against the disease began in 1928 with the foundation of a National Tuberculosis Sanitorium, in which the number of beds has risen from 30 to 105. Previously the only accommodation for tuberculosis

were some rooms in the Hospital Rosales of El Salvador. In 1931 the local Red Cross opened the first public dispensary and founded the National Anti-tuberculosis League. Five years later another dispensary was opened at Santa Ana. In 1939 the director of health created a Department of Tuberculosis and in 1940 opened a new dispensary where 14,000 examinations were made and 93 per cent of the contacts were investigated, while 5,000 homes were visited. In 1938 the first census was taken to determine the amount of infections among school children in the capital; 4,151 were examined. The Montoux and Pirquet tests were found to be positive in 32·8 per cent of the five to six year olds, and in 62 per cent of those aged 14–15. Similar results were obtained in Santa Ana in 1939. Of 12,000 apparently healthy persons at El Salvador in a population of 100,000, 1·42 per cent showed signs of active tuberculosis. Recent figures showed a distribution of 22 per cent, 64 per cent and 14 per cent among the minimal, moderate and advanced lesions respectively, which are very encouraging figures compared with those of 1932, when about 72 per cent of the cases showed advanced lesions.

Suppressing High-Frequency Disturbances from Telephone Apparatus

In an article by M. E. Krom (*Bell Lab. Rec.*, **20**, No. 10, June 1942) it is pointed out that disturbances such as those arising from the opening of the contacts of a telephone dial may be represented by an infinite number of alternating currents extending upwards through the entire radio spectrum. Disturbances of this type may reach receivers by direct radiation, by coupling between the telephone line and receiving antenna, or by secondary coupling via some outside electrical system such as a power line. Two general types of suppression devices are available to limit noise influence. One, located at the source, tends to reduce the radio-frequency voltages developed; the other—usually remote from the source—is functionally a low pass filter. The former type most commonly consists of a small inductance and capacitance connected across the dial contacts. The inductance prevents too rapid a build-up of current as the contacts close whilst the capacitance reduces the voltage on the telephone line as they open.

The filter action results in the voltage increasing slowly as the contacts open, instead of producing a series of high-frequency surges, and 40–55 db. suppression is obtained. Another type of filter similar in action but of different characteristics, is used to suppress radio-frequency voltages from teleprinters and affords some 50–60 db. suppression over the range of from 200–30,000 kc. The disturbances from power plant associated with telephone equipment are usually effectively suppressed (20–25 db.) by simple condensers. Where the individual sources of noise are numerous the second general type of device—the low pass filter—is used, and it may consist of an inductance connected in series with each side of the line, the outgoing ends of which are terminated by a condenser connected to earth. It is desirable that inductive elements used in radio filters have a high effective resistance over a wide band of frequencies instead of a peaked impedance at the resonant frequency.

Man-Made Radio Interference

An article by R. A. Shetzline (*Bell Lab. Rec.*, **20**, No. 10, June 1942) reviews the factors involved in this increasingly important subject. Opening with a brief reference to the multi-frequency disturbances caused by the operation of a relay or switch, the author cites an example of a telephone line and an adjacent power line as a source of interference to a nearby radio receiver. Attention is directed to the signal-to-noise ratio of a receiver as the criterion of its performance, and it is shown how this ratio is dependent on the field intensity of the received radio signal and on the effectiveness of the antenna, the latter being a function of the effective height of the antenna and the impedance relations between antenna and receiver.

Expressions are evolved from which it becomes apparent that the best radio reception is obtained when the field intensity, antenna efficiency, line coupling loss and the attenuation along the line are greatest; the latter depends upon whether the line be cable or open wire. Interference by direct radiation becomes of importance only when the source is close to the antenna or on the same premises as the receiver. Experience has shown that in many instances noisy reception reports arise only when distant stations are sought. With field strengths of reasonable values, and with suitable antenna circuits, simple preventive measures should mitigate the annoyance caused to radio listeners by telephone apparatus.

The Night Sky in September

The moon is new on September 10d. 15h. 53m. U.T. and full on September 24d. 14h. 34m. The following conjunctions will occur during the month: Sept. 2d. 20h., Saturn in conjunction with the moon, Saturn 3° N.; Sept. 6d. 00h., Jupiter in conjunction with the moon, Jupiter 4° N.; Sept. 9d. 05h., Venus in conjunction with the moon, Venus 2° N.; Sept. 9d. 15h., Venus in conjunction with Regulus, Venus 0·7° N.; Sept. 12d. 15h., Mercury in conjunction with the moon, Mercury 6° S.; Sept. 30d. 05h., Saturn in conjunction with the moon, Saturn 3° N. Mercury, in greatest elongation, 27°, on Sept. 15, is an evening star and unfavourably placed for observation for northern observers. Venus is a morning star, and Jupiter in Gemini is well placed for observation in the morning. Saturn and Uranus are not very far from Aldebaran and appear in the east before midnight. No occultations of any bright stars will occur during the month. The longer evenings provide an opportunity for seeing many interesting sights such as the Pleiades, the variable star Algol, the Great Nebula of Andromeda close to the star ν Andromedæ, and other objects of interest.

There will be a partial eclipse of the sun on Sept. 10, visible at Greenwich. The circumstances of the eclipse at Greenwich are as follows:

Magnitude	Eclipse begins	Greatest phase	Eclipse ends
0·33	15h. 24m.	16h. 17m.	17h. 07m.

Announcements

Sir Henry Dale, who was recently appointed a member of the Governing Body of the Lister Institute of Preventive Medicine, has been elected by the Board as its chairman.

An open meeting of the Association of Scientific Workers will be held on September 4 at 7 p.m. at the Caxton Hall, London, S.W.1, to discuss and support proposals for a Central Scientific and Technical Board. The speakers will include Lord Strabolgi.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Non-effect of Massive Doses of Progesterone and Desoxycorticosterone on Lactation

UNTIL recently, progesterone has been generally regarded as devoid of mammary growth stimulating activity in most species. Its positive action in the mouse appeared to be exceptional[1], though doses of 4 mgm. daily were without effect in the rat[2]. It has lately transpired, however, that massive doses (15 mgm. daily) promote mammary development in the rat[3,4]. Two progesterone derivatives, desoxycorticosterone (21 - hydroxy - progesterone)[5,6], and pregneninolone (anhydro-hydroxy-progesterone)[7] have also recently been shown to exhibit mammogenic activity.

In 1937, Folley and Kon[8], on the basis of existing evidence, put forward the generalization that substances showing mammogenic activity also possess the power to inhibit established lactation. The question therefore arises whether or not this generalization is in accord with the new facts mentioned above. The effect on lactation in rats of the three steroids in question was accordingly investigated.

The experimental technique and method of studying lactation were as described previously[8,9], facilities for working with rats being kindly provided by Dr. S. K. Kon. Progesterone and desoxycorticosterone acetate (DOCA) were given by subcutaneous injection in sesame oil (1 ml. daily). The administration of large doses of pregneninolone presented some difficulty on account of its relative insolubility. For this reason the maximum daily dose that could be conveniently given was 4 mgm. dissolved in 2 ml. of propylene glycol. In the first series of experiments (January 1942), ten consecutive daily doses were given and in the second series, eleven, in all cases beginning on the sixth day of lactation. The results are summarized in the accompanying table.

It is evident that in the experiments with progesterone and desoxycorticosterone acetate no detectable inhibition of lactation occurred. Folley and Kon[8] had already shown that administration of 1 mgm. progesterone daily to rats had no inhibitory effect on lactation; it is interesting now to find that in rats from the same colony doses fifteen times as great are equally without effect. It may be noted that the dosage per unit of body weight in these experiments is smaller than that which Selye[3] used to obtain mammary growth, since his rats weighed only 108–141 gm., but mammary growth was reported by Reece and Bivins[4] using the same dosage in rats weighing 217 gm.

The negative results with 5 mgm. and 10 mgm. of desoxycorticosterone acetate daily are rather surprising, since Gaunt[10] in experiments on adrenalectomized rats obtained slight evidence that desoxycorticosterone inhibits lactation. However, in intact rats receiving 0·5 mgm. daily, he observed no inhibition.

Only with pregneninolone was there any evidence of interference with lactation. In this group the growth curve of the pups underwent a slight depression beginning immediately after the injections were instituted.

It might appear from these results that the generalization proposed by Folley and Kon is no longer tenable. It is possible, however, that the threshold for the mammary growth response may be lower than for inhibition of lactation and that administration of even higher doses of these steroids might give positive results. These results are published now since they are of some interest and because further experiments with higher doses of progesterone are unlikely to be possible in the near future.

Date	Mothers: Treatment	Mothers: No. in group	Mothers: Mean wt. on 6th day of lactation (gm.)	Litters: Total No. of pups on 1st day of lactation*	Litters: No. of pups weaned	Litters: % weaned	Litters: Mean wt. at weaning (gm.)
Jan. 1942	Sesame oil (control)	4	303	32	31	97	42·9
,, ,,	10 mgm. progesterone daily	4	234	31	29	94	38·4
,, ,,	5 mgm. DOCA daily	4	263	31	30	97	42·3
May 1942	Sesame oil (control)	4	230	32	29	91	40·7
,, ,,	15 mgm. progesterone daily	3	250	24	22	92	40·7
,, ,,	10 mgm. DOCA daily	3	237	24	22	92	37·6
,, ,,	4 mgm. pregneninolone daily	3	240	22	22	100	35·4

* Litters were reduced to eight at birth.

I am indebted to Messrs. Organon Laboratories Ltd. for progesterone and desoxycorticosterone acetate, to Messrs. Ciba, Ltd., for desoxycorticosterone acetate and anhydro-hydroxy-progesterone and to Messrs. Schering, Ltd., for progesterone provided before the War.

S. J. FOLLEY.

National Institute for Research in Dairying,
Shinfield, near Reading.
Aug. 12.

[1] Gardner, W. U., and Hill, R. T., *Proc. Soc. Exp. Biol. N.Y.*, **34**, 718 (1936).

[2] Selye, H., Browne, J. S. L., and Collip, J. B., *Proc. Soc. Exp. Biol. N.Y.*, **34**, 472 (1936).

[3] Selye, H., *Proc. Soc. Exp. Biol. N.Y.*, **43**, 343 (1940).

[4] Reece, R. P., and Bivins, J. A., *Proc. Soc. Exp. Biol. N.Y.*, **49**, 582 (1942).

[5] Van Heuverswyn, J., Folley, S. J., and Gardner, W. U., *Proc. Soc. Exp. Biol. N.Y.*, **41**, 389 (1939).

[6] Speert, H., *Bull. Johns Hopkins Hosp.*, **67**, 189 (1940).

[7] Mixner, J. P., and Turner, C. W., *Proc. Soc. Exp. Biol. N.Y.*, **47**, 453 (1941).

[8] Folley, S. J., and Kon, S. K., *Proc. Roy. Soc. Lond.*, B, **124**, 476 (1938).

[9] Folley, S. J., *J. Physiol.*, **93**, 401 (1938).

[10] Gaunt, R., *Proc. Soc. Exp. Biol. N. Y.*, **47**, 28 (1941).

Collection of Eggs from the Fallopian Tube of the Rat

DURING the course of recent experiments in collaboration with Mr. P. C. Williams, an account of which will be published later, it has been noted that ovulation and superovulation can be produced in both intact and hypophysectomized immature rats by a single injection of mare serum gonadotrophin followed by a single injection of chorionic (urinary) gonadotrophin. In hypophysectomized rats, ovulation takes place 13–14 hours after the administration of the luteinizing hormone (chorionic gonadotrophin). The number of eggs produced may be very large; in one rat no less than forty-nine eggs were present in the Fallopian tubes.

In keeping with the observations of Long[1], I have examined the Fallopian tubes of such rats in the fresh condition in order to avoid the cutting of a large number of serial sections, and to facilitate the rapid collection of the eggs. The method adopted is as follows: The Fallopian tube is uncoiled by microdissection. When superovulation has occurred, macroscopic observation reveals a characteristic swelling or bulging of a short length of the tube near its ovarian end. This swelling, visible to the naked eye, has a glassy appearance which is in sharp contrast to the opacity of the remainder of the tube. Under a binocular dissecting microscope the swollen part of the tube is seen to contain the eggs (Fig. 1). Each egg is surrounded by a dense mass of cumulus cells, and the cell-masses are held together by a translucent gelatinous fluid (Fig. 2). Ovulation of the normal number of eggs does not produce a cell-mass big enough to cause this swelling, though the binocular microscope enables the eggs to be seen quite easily in the tube, as described by Long. The eggs are collected by excising the egg-containing segment of the tube, and applying gentle pressure at one end of the segment to squeeze out the contents.

Fig. 1.

FALLOPIAN TUBE OF HYPOPHYSECTOMIZED RAT AFTER SUPEROVULATION, SHOWING THE SWOLLEN EGG-CONTAINING SEGMENT. (× 15)

Long removed the surrounding cumulus cell-mass and corona radiata by immersing the eggs in a sperm suspension, but the recovery of the eggs is then made difficult by the opacity of the fluid. I have made use of the observations of Yamane[2] and Pincus and Enzmann[3] that the dispersal of the follicular epithelial cells surrounding the egg can be effected by the supernatant fluid from a centrifuged suspension of spermatozoa previously killed by heat, or by a solution containing trypsin. The former of these methods has been applied with success. A sperm-free solution,

Fig. 2.

CONTENTS OF THE SWOLLEN SEGMENT, ABOUT 20 EGGS SURROUNDED BY CUMULUS CELL-MASSES CAN BE COUNTED. (× 15)

prepared in this way, causes disintegration of the cumulus and corona radiata surrounding the egg of the rat in about one hour. The eggs, now surrounded only by the zona pellucida, can then be removed with comparative ease from the medium.

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August 7.

[1] Long, J. A., *Univ. Cal. Pub. Zool.*, **9**, 105 (1912).
[2] Yamane, J., *Cytologia*, **1**, 394 (1930).
[3] Pincus, G., and Enzmann, E. V., *J. Exp. Zool.*, **73**, 195 (1936).

Vitamin C in Walnuts

THE fact that unripe walnuts (*Juglans regia*) contain very large amounts of vitamin C appears to have been first reported by Gergelezhiu[1] in 1937. This remarkable observation that a fruit as large as the walnut contains 1,000–3,000 mgm. of vitamin C per 100 gm. appears to have been overlooked in English-speaking countries although the findings have since been confirmed by German workers[2]. It has been shown by the Russian author that the vitamin C content is at its highest at the stage of development when the kernel is soft and just before the formation of the shell.

TABLE 1

VITAMIN C IN DIFFERENT SPECIES OF WALNUTS

	mgm. per 100 gm.
Juglans regia—source A	1460–1800
—source B	1520–1640
Juglans regia v. *sinensis*	1120
Juglans cathayensis	860
Juglans nigra	700
Juglans cordiformis	680
Juglans rupestris	510
Juglans mandshurica	410

We have recently examined a number of species of walnuts. It can be seen from Table 1 that of these the cultivated edible walnut contained the highest concentration of vitamin C. The figures for the less common Juglans species must be taken as approximate since it was not possible to ensure that the specimens were analysed at the stage of development when the vitamin content was highest.

The analyses shown in Table 1 were carried out on the whole fruit by the usual titration with 2.6 dichlorophenol indophenol. In order to verify the specificity of the method for green walnuts the following tests were done. Although Harris and Olliver[3] have shown that tannins are unlikely to interfere with the vitamin C determination, Gatti and Knallinsky[4] have reported that such substances reduce the dye. Precipitation of walnut extracts with gelatin and acid salt solution, which completely removed the reducing activity from tea extracts, caused no reduction in the apparent vitamin C concentration of the walnuts for 30 minutes. Furthermore, treatment of walnut solutions with ascorbic oxidase prepared from cucumber[5] caused a loss of 98·4 per cent of their activity. The high indophenol-reducing properties of walnuts appear, therefore, to be due to vitamin C.

In order to discover whether this surprising concentration of vitamin C was a characteristic of the walnut or was shared by other nuts a number of different types were examined. All these nuts were in an unripe condition similar to the walnuts.

TABLE 2

VITAMIN C IN UNRIPE NUTS

		mgm. per 100 gm.
Juglans regia	walnut	1460–1800
Pterocarya Rehderiana	Rehder's wingnut ..	1540
Carya ovata v. *pubescens*	a hicory	820
Carya ovalis v. *obcordata*	,, ...	580
Carya glabra	,, ...	460
Pterocarya sternoptera	a wingnut	440
Carya alba	a hicory	70
Æsculus Pavia	a horsechestnut ..	20
Prunus Amygdalus	almond	20
Æsculus Hippocastanum	common horsechestnut	8
Æsculus carnea	red horsechestnut ..	5

In spite of the active enzyme systems in walnuts which cause rapid blackening when cut surfaces are exposed to the air, Gergelezhiu found that the vitamin C is remarkably stable. The methods commonly used in the U.S.S.R. for preparing sweet confections from walnuts, however, destroy all the vitamin C. The pricking and brining, followed by exposure to sunlight or treatment with steam used in making the pickled walnuts popular in Great Britain also completely destroy all the vitamin content. If, however, walnuts are pickled by either of two methods published by Mrs. Hannah Glasse in the eighteenth century the whole of the vitamin C is retained. Walnuts can also be used as a source of vitamin C in the diet in the form of jam or chutney.

TABLE 3

UNRIPE WALNUTS AS A SOURCE OF VITAMIN C IN THE DIET

	Vitamin C mgm. per 100 gm.
1. *Green pickled walnuts.* Unripe nuts peeled very thinly; soaked for 24 hours in 15 per cent salt solution; packed in spiced vinegar (without cloves); left for 3 months	1080
2. *White pickled walnuts.* Unripe nuts peeled until white; soaked for 12 hours in 15 per cent salt solution; transferred to a second salt solution and simmered for 5 minutes; packed in spiced vinegar (without cloves); left for 3 months ..	1070
3. *Walnut marmalade.* 230 gm. unripe nuts sliced into 560 ml. boiling water; boiled until soft (40 min.); 340 gm. sugar added, boiled until set (20 min.)	630
4. *Walnut and blackcurrant jam.* Jam made with 453 gm. nuts, 112 gm. blackcurrants and 560 gm. sugar	520
5. *Walnut and green tomato chutney.* 680 gm. nuts, 453 gm. green tomato, 225 gm. shallots, 225 gm. raisins, 225 gm. sugar, 5 gm. mustard seed, 15 gm. salt, 880 ml. vinegar, 25 ml. saccharine solution ..	410

The samples of pickles, of which the analyses are given above, were made in 1940. The determination of vitamin C was carried out in August 1942. When it is considered that orange juice contains about 45, rose-hip syrup 200 and blackcurrants 300 mgm. vitamin C per 100 gm., the concentration in walnuts pickled as indicated above is very remarkable. A single walnut weighing 15 gm. will provide 150 mgm. of vitamin C, or at least twice the maximum daily requirement.

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RONALD MELVILLE.
Aug. 5. HENRY SARSON.

[1] Gergelezhiu, *Bull. App. Bot., Genetics, Plant Breeding, U.S.S.R., Supp.* 84, 2, 206 (1937); *Konservnaya Prom.*, **4**, 15 (1938); *C. R. Acad. Sci., U.S.S.R.*, **21**, 246 (1938); *Khim. Referat. Zhur*, 2, No. 1, 50 (1939).
[2] Kennig and Ohske, *Biochem. Z.*, **306**, 16 (1940).
[3] Harris and Olliver, *Biochem. J.*, **36**, 155 (1942).
[4] Gatti and Knallinsky, *Z. physiol. Chem.*, **263**, 37 (1940).
[5] Meiklejohn and Stewart, *Biochem. J.*, **35**, 761 (1941).

Mating Swarm in a Chironomid, *Spaniotoma minima*

A DIFFICULTY often encountered in the study of the habits of mosquitoes is the failure of many species to mate in confined spaces in the laboratory. To overcome this difficulty larger and larger chambers have usually been constructed to gain space for the formation of a mating swarm (Hackett[1]). Lloyd[2] notes this difficulty with the chironomid, *Spaniotoma minima*, one of the common sewage flies. In the course of recent investigations I have succeeded in overcoming the inhibition in this species without enlargement of the insectary, and the conditions found to limit the mating of this insect may have possible applications to malariological problems.

The insects, which are positively phototropic, were found to mate very freely in small glass bell-jars, if the light was not decidedly from one side. Screening the window light and placing an electric lamp directly over the bell jar resulted in a typical dance, accompanied by free mating. In a glass box, 20 in. × 10 in. × 12 in., swarming was most active when the source of light was directly above, but swarming and mating also occurred with illumination from below and also, though less freely, when the four sides of the box were equally illuminated. All round, rather than unilateral, illumination would seem to be essential, therefore.

Below 10 foot-candles, swarming in laboratory cages declines with decreasing light intensity. In the field the flies are found swarming in early morning so soon as it is possible to perceive them and before the photometer will register (that is, below 0·25 f.-c.). The swarms increase with increasing light and in calm weather the flies may be seen dancing in great abundance in bright sunlight, so that swarming occurs over a wide range of light intensity. The lower temperature limit for swarming is 46° F. and for mating 48° F. There appears to be no upper limit for either process under natural conditions. Swarming has been seen to take place on hot, dry days in summer and also during fog and rain. Relative humidity therefore seems to have no influence on its occurrence. When temperature is favourable, wind is the most important factor governing the occurrence of swarms in the field. At velocities below 50 ft. a minute swarming is at a maximum and is progressively reduced up to 200 ft.

a min. (2·27 m.p.h.), which is about the inhibiting force. When wind is of interfering velocity, shelter is chosen wherever possible, but, other things being equal, areas of brightest illumination are preferred. Swarms are therefore found (1) over patches of lighter ground colour and reflecting areas (for example, sheets of water), and (2) behind barriers which give shelter or over raised objects which set up eddy currents counteracting the main wind force.

The sexes are equally abundant, but, as is usual with swarming chironomids, males predominate to some 93 per cent in the established swarm. When, however, a swarm is freshly induced in the laboratory, the proportion of females is at first high, but since the males return to the swarm after mating, whereas the females do not, the customary sex proportion (93 per cent males) is soon attained. Single females or single males will dance freely with the same characteristic motions.

Swarming does not appear to condition the reproductive organs of the males for copulation, and no change is found in the distribution of sperm after dancing. The purpose of the swarm appears to be to bring the sexes of a species together, since the various species of swarming Diptera form dances characteristic in height and pattern, and probably also to stimulate the insects to the necessary pitch of excitement for successful copulation.

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Leeds. Aug. 1.

[1] Hackett, L. W., *Trans. Roy. Soc. Trop. Med. Hyg.*, **28**, 109 (1934).
[2] Lloyd, Ll., Graham, J. F., and Reynoldson, T. B., *Ann. Appl. Biol.*, **27**, 122 (1940).

Cambridge Scientific Workers and the War

IN an article under the above heading[1], the following passage occurs in reference to research topics suggested or approved by the Advisory Research Council of the Chemical Society: "It was stated that the majority of these problems are long-term problems related to the improvement of the post-war prospects of the chemical industry. . . ."

No such statement has been made, by me as chairman of the A.R.C.C.S., or in any authorized pronouncement of this body. The topics adopted cover a very wide range and many are directly related to the war effort or to conditions arising from the War; a considerable number have a chemotherapeutic or medical interest. In a number of cases the investigator has been put into direct touch with a Government department and an extra-mural research of that department has been started as a result. On the other hand, it is true that we have accepted suggestions made by the chemical industry, and we have done this in the belief that stagnation must result unless development is continuous.

The organic chemical industry, like the human body, has certain permanent elements, but it also includes much that needs rejuvenation and constant improvement. It must be emphasized that all the work of this kind, sponsored by the Council, has been suggested by industry itself; we have initiated none of it.

It is true that studies of industrial interest have been made, with great success, at Cambridge, and an explanation on the above lines has probably been given. But it is an argument from the particular to the general to conclude that all, or most of, the work recommended by the A.R.C.C.S. is of this character.

Just as Mr. Churchill declared that the popular demand for a second front was a healthy sign, so we must all sympathize with the agitation for the better use of scientific workers in the War without necessarily agreeing that all the panaceas put forward are feasible. The subject bristles with difficulties and it is often forgotten that a considerable scientific manpower is already available to the Ministries in their research establishments and existing extra-mural contacts with university and industrial laboratories. Obviously it is only a residuum of worth-while *ad hoc* problems that cannot be tackled. During a war we have to find men for jobs, not jobs for men.

It is equally clear that the directors of official research and their assistants and consultants have no monopoly of originality, initiative, foresight, or knowledge; no one of them would claim that he must have thought of everything. Therefore, the pressing need is not for the farming out of specific tasks but rather for the elaboration of a mechanism of contact so that fresh minds and new ideas may be brought to bear on our problems.

Failing such opportunities for present service, need anyone be ashamed of pursuing what is strangely called 'academic' research? Students must be trained in technical subjects and consequently must have teachers. If the latter can find war work to do in addition, it is all to the good; if not, they are still useful and necessary members of the community and it is their duty to keep themselves intellectually alive by making contributions to knowledge. Experience shows that the enthusiasm of undergraduates is fully evoked only by direct contact with teachers inspired by the spirit of active investigation.

A speaker at the conference, of which the article referred to is a summary, defined 'war research' as work which will *very probably* give results applicable to *this* war. Earlier it is stated that junior members of university laboratories should be provided with more of the right sort of problems to solve. The combination of these views implies a serious criticism of the scientific activities of the Ministries which would be hard to justify. Is it suggested that there exists a mass of clearly useful short-term projects which has been neglected? If it is so, the remedy should be the strengthening of the official technical establishments (possibly extra-murally) because, once a short-term desideratum has been recognized, it must be attacked energetically and cannot be left to unconnected volunteers. Therefore we must envisage an expansion of extra-mural contracts with the universities, and teachers outside these arrangements should make independent efforts to get inside them. Failing this, real research by teachers is real national service.

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August 9.

[1] NATURE, **150**, 186 (1942).

WE are glad to publish Sir Robert Robinson's communication since, as he points out, individual men of science and, indeed, scientific bodies, in their enthusiasm, seem to forget that much is already being done both within and outside the various Ministries. We feel, however, that during a war we should find not only men for jobs but also jobs for men. The Central Register is now extending its activities to cover the latter.—EDITORS.

NATIVE PROTEINS, FLEXIBLE FRAMEWORKS AND CYTOPLASMIC ORGANIZATION

By DR. DOROTHY WRINCH
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A REVIEW of recent work in physical, physiological and enzyme chemistry, and crystallography[1] yields a rather definite and precise picture of the native protein. In consequence of the widespread phenomenon of the reversible dissociation of native proteins into aliquot parts[2], it is necessary to assume that the native proteins, consisting in some cases of molecules in monomeric form—which we shall call protein units to indicate that they cannot dissociate further without losing their protein character—are in many cases particles, consisting of aggregates of protein units[3]. The protein unit, it appears, is a rigid atomic framework formed by the interbonding of the skeletal atoms of the residues. In consequence the C_α atoms carrying the amino acid *R*-groups and the C_α—N atoms holding the imino acid rings form fixed patterns in space. It is, we suggest, the spatial patterns of the N—C_α—C atoms in the skeletons which define the protein character and the allocations of this and that variety of residues to the various positions in these skeletons which define the specific characters of individual units. Such a picture, suggested by data from so many fields, seems definitely required by immuno-chemical findings[4], which make it necessary to postulate that the specific *R*-groups on protein antibodies are held in spatial patterns on the molecular surfaces[5]. To complete the description of protein particles and protein frameworks in general, it is necessary, in addition, to specify the way in which the individual protein units—which need not be identical—are associated. It is the nature of these associations and the implications regarding the nature of systems of native protein units in general which are the subject of this communication.

It seems probable, as has often been suggested by protein chemists, that the units are in some cases held together by means of covalent bonds, for example, when a cystine residue is knitted into the skeleton of two units or when peptide bonds are formed between basic and acidic *R*-groups of different units. Evidence as to the mild treatments which disrupt some protein particles indicate, however, that in many cases the association is not of this type, and a study of structure analyses of crystals of molecules

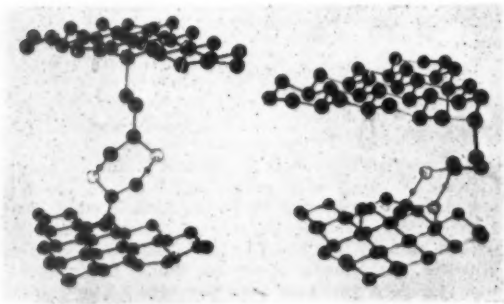

Fig. 1.

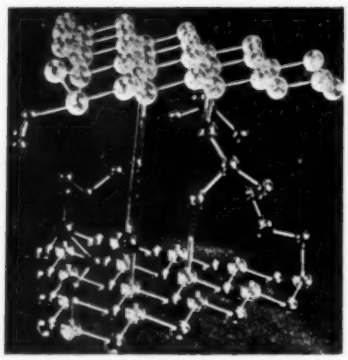

Fig. 2.

containing groupings of the types present in the *R*-groups of proteins provides a number of precise suggestions as to possible ways in which associations can occur. We find two different kinds: by means of polar links and by the apposition of comparatively non-polar groups[6].

The first set includes the following, each based upon a precedent established by structure analyses. (I) Association by hydrogen bridges of glutamic or aspartic acid *R*-groups through their carboxyl groups, with or without the intermediary of 'foreign' molecules (for example, waters). (Precedents: formic acid, oxalic acid and oxalic acid dihydrate.) (II) Association by hydrogen bridges of residues the *R*-groups of which contain COO^- and NH_3^+ groups. (Precedents: glycine, alanine.) (III) Association of *R*-groups containing hydroxyls, with or without metallic ions as intermediates. (Precedents: sugars and metallic hydroxides.) On the basis of the distribution of oxygen atoms around metallic ions in the latter cases, similar associations of COO^- groups on glutamic or aspartic acid residues may also be suggested. (IV) Association by hydrogen bridges of acid amide residues through their CO—NH_2 groups, as shown in Fig. 1, in which the apposed portions of skeletons are indicated diagrammatically by laminæ of atoms. (Precedent: acetamide.)

Suggestions regarding the second type of association, depending upon van der Waals attractions between less polar groups, can be obtained from a study of hydrocarbon crystals. As an example, Fig. 2 shows a possible (trigonal) arrangement of leucine residues on apposed faces, terminal methyl groups from each surface fitting into pockets formed by terminal methyl groups on the other.

The considerable surface areas on native proteins can thus be held together, provided enough groupings are associated. An array of trigonally arranged glutamine residues, for example, on each of two apposed surfaces, could interlink somewhat as in the acetamide crystal. This and the leucine case provide examples of the way in which *R*-groups, *when rooted in an atomic fabric*, can form surfaces of high specificity and how such patterned surfaces can complement one another, as occur in immunological phenomena[5].

The association of homologous chromosomes may prove capable of interpretation in terms of such patterns, in the special case in which they are not only complementary but also self-complementary. Such interrelations between surfaces, permitting the simultaneous associations of groups each no stronger than

those holding together molecules of normal size, but all together adding up to a considerable sum, would seem to be one of the essential factors in the crystallization of megamolecules, as well as in the formation of megamolecular particles[7], including antigen-antibody complexes[5].

Both types of association may also be expected to take place intramolecularly, thereby stabilizing the native protein structure. That such associations (for example, of acid amide groupings) may be a major factor in stability is indicated by recent studies[8]. When they take place intermolecularly, the various orientations and degree of extendedness of the *R*-groups (see Fig. 1) give a certain flexibility to the particle. Just as a molecule consists of atomic units held together by covalent bonds, so a protein particle consists of protein units held together by the association of skeletal atoms and/or *R*-groups, but whereas the strongly bonded atoms hold each other at definite distances in definite directions, the native protein units associated through their polar or non-polar groupings do not. Such associations allow them to lie at various distances apart up to a fixed maximum and in a variety of orientations. I suggest that this aspect of the possible types of associations of native protein units gives the key to the 'plasmolytic' capacity of many native proteins to form crystals containing very different water complements[9]. They also suggest an explanation of the nature of cytoplasmic organization and of the many 'anomalous' properties of protoplasm.

It should also be pointed out that this idea of cytoplasmic organization as depending upon flexible frameworks of native protein molecules leads at once to a simple hypothesis as to the nature of biologically active membranes. On this picture they are to be regarded as *molecular fabrics*, that is, as two-dimensional arrays of native protein units, interlinked by *R*-groups, possibly with some ions[10] and other foreign molecules (water, sugars, lipids, etc.) as intermediates: possibly as two or more of such fabrics superposed. Points at which the native protein units are interlinked by groupings of non-polar character will then be permeable, highly selectively, to non-polar molecules. The open structures in the neighbourhood of interlinking *R*-groups of polar type, the stereochemical requirements of which prevent a closely packed arrangement, will permit the passage of water molecules and of polar molecules of suitable size, shape and nature. In particular, the type of association suggested in (III), following the study of the possible intermolecular links in the insulin crystal, would seem to throw light on some of the striking facts regarding the effect of, for example, copper in very low concentrations on biological membranes[10].

Detailed suggestions as to the nature of these various types of flexible frameworks which appear to form an essential feature of living systems, including the biologically active membranes and immune bodies, will be published shortly.

[1] Wrinch, *Cold Spring Harbor Symposia on Quant. Biol.*, **9**, 218 (1941).
[2] Svedberg and Pedersen, "The Ultracentrifuge" (Oxford, 1939).
[3] Wrinch, *Cold Spring Harbor Symposia on Quant. Biol.*, **6**, 122 (1938).
[4] Wrinch, NATURE, **137**, 411 (1936).
[5] Marrack, "Chemistry of Antigens and Antibodies" (London: H.M. Stationery Office, 1939), 27, 91.
[6] Wrinch, *Collecting Net*, **16**, 121 (1941).
[7] Wrinch, *Phil. Mag.*, **26**, 313 (1938); **31**, 177 (1941).
[8] Conn, Gregg, Kistiakowsky, and Roberts, *J. Amer. Chem. Soc.*, **63**, 2080 (1941).
[9] Fankuchen, *Ann. New York Acad. Sci.*, **41**, 157 (1941).
[10] Wrinch, *Collecting Net*, **16**, 177 (1941).

THE BRITISH RHEOLOGISTS' CLUB

THE seventh general meeting of the Club was held at the laboratories of the Electrical Research Association, Perivale, Middlesex, on July 25, Dr. Stanley Whitehead being in the chair.

A discussion on the "Design and Standardisation of Rheological Apparatus" was introduced by Mr. J. G. Gage. Mr. Gage quoted Bingham's remark that the apparatus used in the young science of rheology corresponded to that used by Faraday in the early days of electrical science. He went on to outline the four main types of flow-curve with which rheologists are familiar, corresponding to Newtonian, pseudo-viscous, plastic (Bingham) and hysteresis systems, in the last of which the flow-curve depends on stress history. He briefly described the principal types of instrument used to study these systems: capillary viscometers, which he regarded as unsatisfactory for thixotropic systems; Couette and Stormer rotation instruments; the multiple jet Gardner mobilometer and its modifications; falling sphere viscometers, etc.

Mr. de Waele disagreed with Mr. Gage's views on the relative merits of capillary and rotation viscometers, claiming that we know more of the hydrodynamics of the capillary tube than of the rotation instrument. He regretted that the work of Rabinowitsch had not been more widely appreciated. In his experience all materials give linear flow-curves if high enough stresses are used and he was sceptical about the existence of sharply defined yield-values.

Commander Goodeve, in discussing thixotropic systems, pointed out that the properties of materials are changed by shearing them, and flow-curves for fully broken-down and completely rested materials cannot be obtained. He explained his "coefficient of thixotropy" and discussed its relation to mobility and yield-value. Dr. Scott Blair stressed the difficulty experienced by rheologists engaged in war-work in getting experimental instruments made when required in small numbers. The intermediate stage between the home-made apparatus and the catalogued standard article was most difficult to bridge. He appealed to instrument makers for help in this matter. Mr. Thomas discussed the rheology of plastic insulating materials and criticized the use of the Martens' test, which he regarded as unsound. Mr. Tayar asked whether the use of an eccentric inner cup in a rotation instrument to prevent "channelling" or slipping was sound in principle.

Dr. White considered that there were many instrument giving useful practical results which were not above reproach on theoretical grounds and that the instrument referred to might be little or no worse than many others. He suggested that the use of integral equations might be of great benefit to rheology. Mr. Plesch stressed the importance of differentiating the degrees of accuracy needed for different instruments. Rheological apparatus used in industry differed widely in this respect. Dr. Wearmouth discussed the rheology of plastics studied over a wide range of temperature, about 200° C. At some temperatures some of these materials showed well-marked yield-values, whereas at others, and often over a wide temperature range, there were no yield-values or the yield-values behaved in a quite erratic manner. The industry wanted a single instrument, if possible, to cover the whole field of behaviour.

Dr. Whitehead, in closing the discussion, pointed out the analogy with the problem of dielectrics and discussed the Hopkinson superimposition principle. He also supported Dr. White's stressing of the importance of integral equations. He mentioned the importance of considering the energy aspects of rheological phenomena and added that he felt that British rheologists were at a disadvantage as a result of the lack of adequate journals for applied physics in Great Britain.

PROTECTIVE MULTIPLE EARTHING

IN 1941 the Council of the Institution of Electrical Engineers set up a special committee to consider the problems arising from the increasing use of multiple earthing of the neutral conductor in factories and other buildings, and the report of the committee has now been published (*J. Inst. Elec. Eng.*, **89**, Pt. 1, No. 19; July, 1942). The report describes briefly the earthing protective methods generally employed in urban and rural areas and in cities, their advantages and disadvantages. Where underground cables are used, the sheathing and armouring of the cables are employed as the earth return path; alternatively, where available, continuous water mains with metal-to-metal joints may serve the purpose. Where neither of these is available, a separate earth electrode can be used at the consumer's premises although often the cost of an efficient earth is disproportionate to that of the electrical installation itself. Another method utilizes the supply company's low-voltage insulated neutral conductor, connexion being made thereto of each consumer's earth-continuity conductor.

Protective multiple earthing consists of the last-mentioned method, with the addition of multiple connexions of the neutral conductor to earth at other positions on the low-voltage network, and of a connexion between the neutral conductor and earth at each consumer's premises; the earth continuity wire may be dispensed with but this is not recommended. Finally, miniature circuit breakers responsive to very small leakage currents are used.

The Committee's recommendations are as follows: (1) In areas where the supply authority's distribution network consists of underground cables, there should not be any relaxation, other than that contained in Regulation 4, of the Electricity Supply Regulations (1937), which requires earthing at one point only. (2) In areas where the supply authority's overhead distribution system is at low or medium voltage, and where it can be ensured that the distribution network and the consumers' installations are under the same control, so far as the earthing arrangements are concerned, protective multiple earthing would provide a satisfactory alternative to existing methods, and might be permitted. (3) Where the supply is separate from the supply authority's distribution network (that is, where a double-wound transformer or private generating plant is used), a multiple-earthed neutral conductor might be permitted subject to specific conditions being imposed to provide against the risk of interference and possible damage to telephone circuits and apparatus, of gas pipes normally carrying current, of water pipes carrying anything but short-duration fault currents, and of light lead sheaths of telephone cables carrying power currents; these and other contingencies are outlined in an appendix.

Three appendixes are given: (*a*) use of a multiple-earthed neutral conductor where the supply is separate from the supply authority's distribution network; (*b*) protective devices responsive to very small leakage currents and actuating circuit breakers on the consumer's premises; and (*c*) definition of "connected with earth".

The report was submitted to the Electricity Commissioners, who expressed the following views. When protective multiple earthing is adopted, all consumers' installations should comply with conditions as to earthing on the lines of those recommended in Technical Report *F/T* No. 122 (1940) of the British Electrical and Allied Industries Research Association; in the case of consumers' installations existing at the date of adoption of such earthing, the responsibility for the installation complying with the conditions should rest upon the electricity undertakers. It would appear that the Wiring Regulations (Nucleus) Committee takes the view that there should be a continuing responsibility on the undertakers in respect of the condition of the consumer's installation, and if this be the case, the Commissioners in making regulations concerning protective multiple earthing will consider carefully to what extent they could properly impose such responsibility on the undertakers. The Commissioners are of the opinion that it is better, in view of the possible danger which the Committee has pointed out, that the earth connexion on live circuits should not be broken, and that no variation in practice in this respect should be made depending on the voltage of the circuit. They have decided, therefore, to make an appropriate amendment of Regulation 4 (iv) of the Electricity Supply Regulations (1937) on the lines of that suggested by the Committee, when a suitable opportunity occurs. When, on the grounds of cost or convenience, a link is not included in the earth connexion, the use of a suitably designed 'clip-on' ammeter should enable leakage tests to be carried out on live circuits without interrupting the connexion with earth.

FORTHCOMING EVENTS

Friday, September 4

Association of Scientific Workers (at Caxton Hall, Westminster, London, S.W.1), at 7 p.m.—Open meeting to discuss and support proposals for a Central Scientific and Technical Board. (The speakers will include Lord Strabolgi.)

APPOINTMENTS VACANT

Applications are invited for the following appointments on or before the dates mentioned:

University Chair of Chemistry, tenable at the Royal Cancer Hospital—The Academic Registrar, University of London, Richmond College, Richmond, Surrey (September 1).

Junior Educational Psychologist at the Child Guidance Centre of the Nottingham Education Committee—The Senior Medical Officer, 28 Chaucer Street, Nottingham (September 2).

Assistant Lecturer in Mechanical Engineering—The Registrar, College of Technology, Manchester 1 (September 7).

Regius Professor of Engineering at Edinburgh University—The Private Secretary, Scottish Office, Fieldon House, 10 Great College Street, London, S.W.1 (September 7).

Head of the Chemistry Department—The Principal and Clerk to the Governing Body, Wigan and District Mining and Technical College, Wigan (September 12).

Harris Chair of Physics, University College, Dundee—The Secretary, The University, St. Andrews (December 31).

Lecturers and Instructors for training Radio Mechanics—The Principal, College of Technology and Arts, Eastlands, Rugby.

Teacher for Day and Evening Engineering Classes—The Principal, County Technical College, Gainsborough, Lincs.